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OBSERVATIONS ON DISEASES OF THE BRAIN, WITH CASES AND DISSECTIONS.

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Case of Extravasation of Blood upon the Surface and within the Medullary Substance of the Brain; the latter probably accidental, and productive of a fatal Termination.

[With an Engraving.]

A. G. a young woman, about 22 years of age, had been for at least a year and a half, a great sufferer from rheumatic complaints, principally in the head. These were attended with a violent head-ach of so great intensity, as sometimes to produce delirium. In one attack, the pains in the head ran on to so alarming an extent, that a temporary state of general paralysis was the consequence, and she lost the use of her limbs. These complaints, however, were, after some time, conquered by the adoption of judicious measures, and within the space of two months she had recovered tolerably well all the powers of motion.

In the month of January, 1810, she left her friends, and came to London from a neighbouring town where she lived. The weather was at the time mild, but finding herself very unwell in the head, she loitered about the streets till the day was spent, and in the evening sat down at a door. About midnight she was desired to go away by a watchman; from this she excused herself by saying that "she could not." Very soon afterwards, however, she was obliged to get up, and with others, walked a considerable distance to the watch-house. Here she remained for the rest of the night, sitting on the cold stone floor. Her companions repeatedly remonstrated with her, endeavouring to persuade her to go and sit down by the fire, and

and to take something either to eat or drink, but to no purpose. All solicitations were useless; she declined, shook her head, and when urged to speak, said she did not want it. To those who were around her, it appeared that she was stupid from intoxication; yet, whenever much teased, she spoke, and that perfectly in reason. Once during the night she said something in reply to an inquiry what ailed her, and observed, that once before she had, from a fit of illness, lost the use of her limbs, and that from her present feelings, she feared that this would soon be the case again.

The following morning, with several other girls, she was taken before a magistrate, to give an account of herself. Those who then saw her could not help supposing her to be still under the influence of intoxication; but the persons who had spent the night with her, declared she had neither drank nor eaten any thing whatever in the night. As nothing satisfactory could be made out, she was brought back, and received into an infirmary, where she remained much in the same state, from the Tuesday night, when she was first brought into the watch-house, till the Saturday evening following, when, notwithstanding every exertion that had been made to save her, she died without any apparent struggle.

From the first she had continued in an uniform dull, silent, and stupid state, though not particularly disposed to sleep. When, with much persuasion, she was once prevailed on to take her medicine, she kept it on her stomach without difficulty.

She had always spoken rationally enough, but was observed to become more and more dull and inattentive to what passed around her, from the hour when she first came in to that of her dissolution.

EXAMINATION.

Upon opening the head, the first appearance of disease was found within the dura mater, which being raised and laid aside, exhibited the pia mater very much altered indeed from its natural state. The membrane was in every part highly vascular, both in the number and magnitude of its vessels. The most remarkable circumstance however, in the condition of the pia mater, was a degree of extravasation of blood, which might be traced all over the surface of almost every part of the brain. The extravasated fluid had formed superficial coagula, corresponding to the spaces between the convolutions of the brain.

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This extravasation of blood appeared to have taken place very universally, not only from the capillary vessels of the pia mater, covering the external surface of each hemisphere, but also from those that form the tomentum cerebri, by dipping down between the circumvolutions of the brain; hence, upon making a section down through the substance of the cerebrum, several coagula of extrasated blood were cut asunder.

That particular extravasation however, which, in all probability, had been the immediate cause of death, was very distinct from the morbid appearances above described. Paring away the superior portion of the right anterior lobe cerebri, a coagulum was found deposited in the very central part of the medulla. In this situation, a vessel had ruptured and poured fourth a quantity of blood to the extent of nearly an ounce.

The engraving represents the coagulum that was discovered in the course of examination in the medullary part of the brain. Its precise size was not ascertained, as it was not disturbed from the cavity in which it was imbedded.

A. The inferior surface of the anterior lobe of the right hemisphere of the brain, where it lays immediately over the orbital cavity.

B. Points out one of those parts of the superior surface of the brain, where the pia mater was detached from its immediate contact with the convolutions, by the deposition of an angular stratum of coagulated blood. This appearance, from its being general upon every part of the surface of the brain was presumed to be the necessary consequence of severe rheumatic inflammation, when it attacks the membranes of the brain with so great a degree of violence, as was manifest in this case.

The circumstance of the extravasation having taken place only on those parts of the surface which mark the separation between the convolutions, may perhaps, be explained upon the principle of the circulating vessels having, in these parts, more freedom of action, and less support; whereas the vessels upon those portions of the membrane more immediately spread over the projecting ridges of the brain, meeting with more support, and to a certain degree even sustaining pressure, are less liable to yield.

This conjecture, however, affords no explanation why the same thing was found to have taken place in situations, where the substance of the brain lay in close contact with the pia mater, because the natural elasticity of

the structure of the brain is the same throughout the whole of its substance.

C. The appearance of the divided portion of a thin coagulated stratum of blood, thrown out from the vessels of the pia mater, dipping down between the convolutions into the substance of the brain.

D. The large deposition of blood scarcely coagulated, that was found in the medullary portion of the brain. From the circumstance of the large coagulum being yet in a gelatinous and almost fluid state, while the superficial coagula, connected with the pia mater, were all of a much more dense, firm, and dry texture; it seems almost more than probable, that the opinion given with regard to their date, is correct.

Case of fatal Apoplexy, in which from the Rupture of Vessels, Blood was effused into the substance of the Medulla Oblongata.

J. M———, an old man, aged 85, was apparently well in health on Sunday, February the 4th, 1810. In the night following he got out of bed to make water. A person who lay near him, being awake, enquired what he was about; to this he replied, that he only wanted to find the chamber pot. These words were no sooner out of his mouth, than he fell suddenly over the bedstead with his head to the floor, which he struck with great violence.

Assistance being obtained, he was raised up and laid in bed again, being in a state of insensibility, but breathing with much freedom, and laying in a state of perfect tranquillity, as if in a light repose.

The next day the pulse was full and hard, beating 92 in the minute; respiration was now rather heavy, and in some degree laborious; but there was no stertor. The eye-lids were still closed, and the jaw perceptibly falling.

On the day following (Tuesday) the respiration was attended with more labour and difficulty, as well as noise; the jaw more fallen, and the pulse slower than it was, but still very full and hard. In the evening he expired.

EXAMINATION OF THE BODY.

The head being opened, the vessels of the pia mater were found in a very turgid state, but they did not appear to have given way any where. The dark colour of the cortical substance in this brain, when compared with the medullary parts, formed a more striking contrast than is often

often seen. Upon examining attentively the section of the cerebrum, a peculiar appearance, certainly of some disease in the extreme capillary blood-vessels was observed. This appearance was universally found but in the cineritious structure only. It resembled very much the appearance of petechiæ, or flea-bites, on the skin; and from the apparent correspondence with the known situation and distribution of the arteries in the cortical substance of the brain, it was concluded that this appearance arose from some diseased alteration in the structure of the capillary arteries, by which a portion of the colouring matter of the blood, just sufficient to stain the circumference of the vessel, was allowed to escape without proceeding to a more sensible degree of extravasation.

This appearance was found in every part of the cortical substance of the cerebrum, but it was more apparent about the basis of the brain than elsewhere.

The lateral ventricle on the right side was of the natural form, and contained only the usual quantity of fluid. On the left side, however, the appearance was very different. The quantity of lymph deposited on this side was equal to between an ounce and a half and two ounces.

The horns of the ventricle had suffered considerable distension from this accumulation of fluid, the discharge of which was not, as on the other side, followed by a collapsed state of the cavity of the ventricle. The appearance after the fluid had run off, was that of a large, extended, vaulted passage, or tube, which to the eye, gave a bold and remarkable impression. This cavity still preserved its natural turns, each of the cornua was equally enlarged with the others, and the whole space retained the exact form into which the contained fluid had moulded it.

In the ventricle on each side the plexus choroides was pale, but little changed from its natural state, except that on both sides this vascular expansion contained several small soft tumours, plentifully supplied with blood-vessels.

Upon examining various sections of the cerebellum, no appearance whatever of extravasation was observed, so that whatever might be the nature of the appearance in the cortical substance of the cerebrum, that of the cerebellum had not undergone any similar change, but was in all respects of healthy structure.

Removing that portion of the cerebellum, which, from its situation, makes the posterior part of the fourth ventricle, several small coagula of blood were observed, laying

on the opposite side, being on the posterior surface of the medulla oblongata.

In order to ascertain the precise extent of this extravasation, the medulla oblongata was first removed from its situation, and then divided by a vertical section, carried from behind, forward. This section exhibited several thin strata of blood, approaching to a state of coagulation.

The disposition of these portions of extravasated blood was remarkable. Many circumstances in the anatomy of the brain favour the idea of the medulla oblongata being made up of a congeries of longitudinal fibres. This appears evidently to be the case, when some portions of its surface are observed with attention. On this supposition, however, it is not easy to understand upon what principle the blood, in escaping from its vessels, should have been disposed, as in this case it was found. Where blood is poured out into any muscular structure, it is known to separate, but rarely, if ever, to rupture the fibres. Should it, on the other hand, be taken for granted, that the structure of the medulla is merely a soft elastic matter, formed without any particular or essential arrangement of its particles, even then effusion of blood into its substance, would, as happens above in the medulla of the brain, assume the form of a mass, more or less solid; but in the present instance, the several very thin strata of blood were all disposed in the same manner exactly, laying transversely to the longitudinal fibres of the medulla, supposing such fibres to exist.

The coagula were found deposited at some small distance apart, one above another, not in a correct line, but so that estimating the probable effect of the whole, it would be a sudden and complete suspension of the functions of the medulla, considering it as the medium of connection between the brain and the system of spinal nerves. The arteries of the brain were, in many situations, completely ossified.

Upon opening into the cavity of the thorax, the heart was found in a very diseased state. The tendinous margin round the insertion of each auricle was converted into a firm mass of bone. The two great coronary arteries of the heart were, from their origin, behind the valves of the aorta, almost as far down as the apex of the heart, completely ossified.

The semi-lunar valves, at the mouth of the aorta, were much enlarged, and distorted from their natural form by the deposition of ossific matter within the margin of their

attachment

attachment to the artery. The whole extent of the arch of the aorta was enlarged to double its natural size, and completely diseased. The inner membrane being deprived of its natural smooth structure by the progress of ossification, was advanced to a very rare extent.

On examination of the internal surface of this artery, many of the ossified masses were found raised into irregular projections, intruding into the cavity of the vessel. The general appearance, however, was that of uniform patches of ossific matter, deposited between the middle coat of the artery and the inner membrane. From the surface of several of the most irregularly formed ossifications, the internal lining of the artery had apparently been removed; in all probability, this had arisen from the irritation of these matters exciting an increased action in the absorbents. Another obvious effect of irritation, produced by these masses of bone upon the coats of the artery, appeared on applying a gentle pressure to several of them, upon which, globules of pus, some of them large, were seen to start out beneath, and some of the scales were found to be by this means, almost quite detached from their situation. Assisted by a lively imagination, the fancy might here have pronounced, that the ever watchful genius, destined to preside over the welfare of the constitution, had directed the commencement of the ulcerative process, under the delusive hope, that by this means it was possible the disease might be removed.

[An engraving, shewing the appearance of the medulla oblongata will be given in a succeeding Number.]

A fatal Case of Apoplexy from Extravasation of Blood upon the Brain.

A labouring woman, in the 56th year of her age, sustained the shock of an apoplectic stroke, which deprived her of her senses and memory for many hours; but from this she eventually recovered the perfect use of her faculties, and was apparently restored to a very good state of health. This happened in the year 1804.

Five years afterward, in 1809, she had a second attack of the same formidable nature; she was suddenly deprived of her senses, and fell down in a state of stupefaction. From this attack also she was partially recovered; but although, after some time, her memory was restored, she never again enjoyed the free and perfect use of her limbs. On the contrary, she was never afterward able to direct her steps with any tolerable precision or even safety.

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In December, 1809, a few months after the second attack, she was afflicted a third time. She had just reached the top of a stone stair case, when she instantaneously dropped down; and such was the ill fortune of the moment, that she fell to the bottom of the stairs. By this accident was received a very violent blow upon the right parietal bone, dividing the scalp, and exposing the bone to some extent. In a state of insensibility she was taken to an hospital, excessively restless, and constantly moaning. The hair being removed, the scalp was found very much bruised, with some degree of laceration. In order that the state of the bone might be more decidedly ascertained, the scalp was divided very freely by a crucial incision; from this it appeared that the skull was neither depressed nor fractured. The pupil of the eye was observed to be in a state of contraction, which was neither altered by the approach of a bright light, nor by leaving the apartment almost in darkness; the iris therefore was in a state of permanent contraction, as well as insensibility. The pulse was in a natural state, beating 76.

Sixteen ounces of blood were immediately taken from the arm, and a cathartic enema directed to be injected as soon as it could be got ready. Thirty drops of the tincture of opium were ordered to be given in a draught in the evening. This woman's condition, during the evening and night, was not that of apoplexy exactly, for she would frequently answer when spoken to, but still spoke incoherently, and always as if impatient from pain.

The following morning the account was very unpromising; there had been considerable hæmorrhage from the mouth, nose, and ears. The injection had returned, soon after being thrown up, without seeming to have produced much action in the bowels. In other respects she was much the same, being still quite incapable of answering rationally any question that was put to her. She seemed disposed to lay quiet as if asleep, yet would instantly open her eyes on being spoken to, but never apprehended what was said to her.

In this state she remained without taking either medicine, or support, that was of consequence, for five days, the pulse throughout remaining in its natural state, not hurried, nor slower than before, neither hardened, nor perceptibly oppressed. About the expiration of the fifth day she expired.

EXAMINATION AFTER DEATH.

The scalp being turned back, the cranium was found without

without any trace of fracture. Upon raising the upper part of the skull an extensive mass of coagulated blood, of a firm, condensed, leathery consistence, was found deposited within the bone, and upon the dura mater. This mass, in extent as well as situation, corresponded very exactly with the temporal space upon the right side of the head, extending downward quite to the basis of the cranium. This blood had nothing of the rich scarlet and purple colour of blood recently coagulated, but was of a reddish brown or chocolate colour.

The dura mater being raised, and a division made at the anterior extremity of the falciform process, in order that the membrane might be with more freedom turned back, the whole of the surface of the left hemisphere of the brain and pia mater was discovered bathed in blood, partly fluid and partly grumous. The hemisphere on the opposite side was altogether free from this appearance. On the surface of the right hemisphere, the pia mater was in many places observed to be separated from the arachnoid membrane by the interposition of a serous fluid, not in any considerable quantity, but sufficient to show the prevailing tendency to lymphatic effusion.

The extravasation of blood was, on further examination, found to have universally diffused itself over every part of the cerebellum, as well as cerebrum, upon the left side. Perhaps the quantity of blood that had formed the more recent extravasation, might have been in the whole equal to three or four ounces.

On opening into the lateral ventricles, the plexus choroides bore the only trace of disease; several little tumors resembling small hydatids were found in each plexus, towards that portion deposited in the descending horn of the ventricle.

The left side of the basis of the cranium could scarcely be seen, from the quantity of effused blood that remained after the lobes of the brain were removed.

The branches of the internal carotid were in several places found to be considerably ossified.

The medulla oblongata and the medulla spinalis were now taken from their cavity, and here the only peculiar appearance met with was in the form of the left vertebral artery. This vessel, at the point where it emerges from its canal in the occipital bone, had become considerably larger than usual, almost to double its proper diameter; but the alteration in texture was more remarkable than even that of its form, the artery being thinner than in its natural

natural state, and of a rigid feel, like dried parchment.— The enlargement of this artery was observable in some degree as far as its junction with the vertebral artery on the opposite side.

It may in this case be reasonably conjectured, that although the third attack, and particularly the accidental fall which attended it, had proved fatal by the pouring out of the large quantity of blood that was found in a fresh and fluid state on examination of the head after death, yet the coagulum within the right temple was the consequence of the second attack. Under these circumstances we see then, the brain had been, from the deposition of this blood, in the first instance, overpowered, and its functions nearly extinguished; but that, assisted probably by the means adopted for its relief, the circulating volume of blood by slow degrees subsided into a smaller compass, so that even with this mass of blood intruding into the space formerly occupied by the brain alone, there was an almost perfect recovery of the faculties, although the decision and force of the voluntary powers were not re-established.

Mill Street, Hanover Square, July 9, 1810.

To the Editors of the Medical and Phyſical Journal.

Gentlemen,

I Beg leave to offer to your consideration the following Remarks on the superior efficacy of Alcohol, as a topical application, where the intention is to remove local inflammation by resolution or dispersion, and particularly that which arises from burning or scalding.

This Essay is the result of much practical observation, and might be supported by numberless cases; but as the subject has already been, in a short way, before the public, and some cases then presented, I shall confine myself within as narrow limits as possible; and should you think it, in this state, deserving of a place in your Medical Journal, I beg you will introduce it in your next.

I am, &c.

T. PARKINSON.

Leicester, June 13, 1810.

A Chemico

A Chemico Medical Essay on the Efficacy of Alcohol, evaporated from Surfaces injured by Burning or Scalding.

By T. PARKINSON, M. D.

Several years ago, I published a small paper in the Medical Journal, on the Efficacy of Spirit of Wine, evaporated from surfaces injured by burning or scalding, and since that time, a great number of cases have occurred to me, which fully confirm the opinion I then advanced.— From a persuasion that it may be extended as a remedy in inflammatory diseases, intended to be removed by resolution, I now resume the subject, with a view to explain the modus agendi in such a manner, as will lead to the most favourable mode of application.

Spirit of wine, or alcohol, has been highly esteemed by the most eminent in the practice of Physic and Surgery, in cases of burning and scalding, under the professed title of an astringent; but is now superseded by the more popular preparations of lead, cold lotions, emollients, and oily liniments.

Before I proceed further, it will be needful for me to inquire, whether inflammation is to be considered a simple uncompounded property, or a compound one.

The symptoms and common causes of inflammation are so well defined and so generally understood, as not to need any notice here; excepting, that it is active inflammation only, to which the evaporation of alcohol is directed; and to divide it into phlegmonous and erysipelatous; the former of which terminates either by resolution, suppuration, or gangrene; the latter either by resolution or gangrene. This consideration must be remembered through the whole of the following observations; and I would particularly notice, that one of the prevailing causes of inflammation is the direct imposition of heat, or the contrary, the abstraction of heat.

I shall, in future, speak of heat under the appropriate term caloric, considering it one of the component parts of life, so far as relates to health, disease, and death.

In health, a relative quantity of caloric, capable, however, of considerable variation, is one of the component parts; beyond the limits of this variation, disease is produced, admitting also of considerable variation; but, beyond these limits death follows.

What quantity is required to sustain life, or what quantity is destructive of it, is not so much our present consideration, as there are limited boundaries, within which, health will be supported; others, within which, disease will

will be produced; and others, beyond which, life will be destroyed.

In no condition, however, can the living fibre endure the heat of boiling water, for any length of time, without being decomposed and destroyed by it; and, may it not, with equal propriety, be said that, in no condition can the living fibre endure a degree of cold, correspondent to the heat of boiling water, without being decomposed and destroyed by it?

Caloric enters into the composition of all bodies, whether animate or inanimate; and is more or less fixed, according to the structure and peculiar properties of each individual body.

Some substances will retain their integrity, and not be perceptibly changed by the greatest abstraction that art can effect; and are, with difficulty, deranged by the imposition of caloric; whilst others assume new appearances and properties by either a slight imposition or a slight abstraction; a more striking instance need not be adduced than the form that metals exhibit, either fluid or solid, according to the quantity of caloric united with them; and more particularly that of quicksilver becoming solid, and even malleable under a degree of abstraction, of which it is capable.

Metals, like other insensible bodies, have not the power of retaining more than their natural or component quantity of caloric any longer than an excess is communicated to them, but presently part with it, until they return to their original temperature, and regain their common appearances and peculiarities: this must, however, be limited to the degree of excess of caloric, that each individual metal is capable of suffering without decomposition.

Earthy and stony substances are also composed partly of caloric, and are subject to changes equally manifest, though not altogether similar to those that happen to metals. Some, however, admit of great excess without any material alteration, whilst others are decomposed by a slight imposition; an instance is notable enough in limestone, which parts with its carbonic gas by the imposition of caloric, and though it was a mild and almost inert substance before, it now becomes active and caustic.

It seems, however, still to prefer carbonic gas, as it will take it from most other substances composed of it, and again let loose the caloric imposed.

Vegetable and animal substances, deprived of life, are decom-

decomposed by a slight abstraction or excess, their other component parts being less fixed or more volatile; but, when they are not deprived of life, they are not so easily destroyed; having the power, when alive, in some measure of resisting decomposition.

In them also a much less excess or abstraction than is required for decomposition is productive of disease; and, less than this constitutes the healthy quantity.

Fluids are very differently affected; for the most part they are converted into steam or vapor by an excess, and into solids by abstraction; and, as it is by the former of these operations that we propose to decompose inflammation, I shall endeavour to explain the manner in which this power is produced by the action of caloric upon fluids.

From what has already been observed, it appears, that inflammation is a compound property; and that caloric, or rather an excess of caloric, is one of the constituent parts. This position is confirmed by observing, tha without such excess, be the other component parts what ever they may, a part cannot be inflamed; that is, th amount of the others is short of inflammation.

Having shewn that an excess of caloric, that is, quantity greater than what can be allowed within th limits of health, is essential to form inflammation, the re duction of such excess seems, primâ facie a direct and e fectual remedy. But speculative reasoning must, at a times, give way to solid facts deduced from practical o servations; I shall therefore take notice of what is par cularly observable on the evaporation of alcohol fro surfaces inflamed by burning or scalding; alleging, th the same applied to active inflammation in general, whatever cause produced, when it is proper to remove by resolution, is highly useful; but must first return to explanation of the manner in which the power of remov inflammation is connected with the evaporation of fluid

The Way in which the Evaporation of Alcohol decomp or destroys Inflammation.

Vapour or steam, like inflammation, is a compou but it is composed only of caloric and a fluid in the atn pheric air, and is the invariable result of the combina of these two agents.

A fluid of a given volatility or chemical attractio caloric, or in other words, disposed to be converted

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steam or vapour, will be dissipated more rapidly than another fluid of less volatility or attraction to caloric; for example, water has an attraction to it, and is proportionately evaporable, under certain circumstances, that is, united with a given quantity. Alcohol has a stronger attraction to caloric than water has; therefore, under similar circumstances, is evaporable at a greater rate. Vitriolic ether has a still stronger attraction; and, therefore, under similar circumstances, is evaporable at a greater rate than alcohol; and the same may be said of all other fluids. The rate then of evaporation, cæteris paribus, will be correspondent to the attraction the fluid bears to caloric.

It must also be observed, that the dissipation of caloric will be in a ratable proportion with the rapidity of the evaporation, viz. Water being evaporated from a surface, in a given time, to the quantum one; alcohol, from the same surface, in the same time, to the quantum two; and ether, from the same suface, in the same time, to the quantum four; the exhaustion of caloric will be with water one, with alcohol two, with ether four. The quantity of cold then induced, or what is the same thing, the quantity of caloric dissipated, will be in the ratio of evaporation, corresponding with the volatility, or evaporability of the fluid employed.

Further, if a piece of iron heated to one hundred degrees, will be cooled by the evaporation of water from its surface in four minutes, we may suppose it will be cooled by the evaporation of alcohol in two minutes, and, by the evaporation of ether in one minute. I do not however, wish to be understood as considering this a correct statement, but have introduced it only to explain, by way of example, the effects necessarily produced by employing fluids of different degrees of evaporability, in the subduction of inflammation. It may not be amiss here to observe, that steam is a compound of caloric and a fluid; and if you abstract the caloric, the product of the remainder is not steam.

On the particular Effects observable on the Evaporation of Alcohol, from Surfaces injured by Burning or Scalding.

Pain is considerably abated at the commencement of the process, and in a short time is wholly removed; but on ceasing the evaporation, is apt to return; is again removed by evaporation; and this alternation may be carried on till inflammation is wholly subdued.

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The precise length of time required for the subduction of inflammation must depend upon many circumstances, which I shall endeavour to point out when I treat on the most eligible method of conducting the process of evaporation.

When there has not been a destruction of the epidermis, though considerable vesication have been produced, there will not need any other application.

When the injured parts have been deprived of their natural covering, there will be required only the application of wax and oil, or any other easy plaister, that is commonly employed in simple ulceration, after inflammation is thus removed.

If the parts are actually decomposed by the imposition of great excess of caloric, neither the remedy here recommended, nor any other remedy, will contribute to their restoration. But yet, the adjoining parts will be subject to much inflammation, which will require to be moderated; and therefore the process of evaporation will be equally beneficial in this, as in that of simple inflammation.

The conveniency and elegance of the process are some recommendation; the philosophical principles with which its efficacy is connected invite some attention; the immediate relief from pain declares its utility; its power, under all degrees of diseases, to which it ought to be applied being always apportionate, bespeaks its superiority; and, the invariable uniformity of its action decides in favour of its preference.

On the Method of conducting the Process of Evaporation.

As the object of our attention is the decomposition of inflammation by the abstraction of one of its component parts, it is required that an appropriate method, as well as the subject of evaporation, be pointed out.

Alcohol being already proposed as the subject, I wish to give it the best possible chance, by applying it in such a manner as will be most favourable to its evaporation; and, as I cannot make out a better, I shall relate that which I have always hitherto adopted.

In cases of inflammation only, a single piece of linen cloth has been laid over the parts aggrieved, so as to extend in all directions, considerably beyond them; alcohol has then been assiduously poured, or what is better, squeezed from a sponge upon it, so as to keep the whole of the linen cloth continually moist with alcohol.

When, however, there is no exposure from a separation of

of the cuticle, it may be evaporated with equal safety and effect from the skin without the interposition of any thing.

When the injured parts have been deprived of their natural covering, a piece of thin bladder, or a plaister of wax and oil, has been laid upon them, well adapted, and then the linen cloth and alcohol, &c.

This precaution is not necessary when the parts are decomposed by great excess of caloric.

Under all circumstances the excess of cold air will contribute much to the efficacy of the process.

As I before observed, the pain is immediately abated; and in a short time, commonly in one hour, wholly removed; it is nevertheless likely to return in a slight degree, but is again promptly relieved by evaporation; and in all cases, and under all circumstances, the recurrence of pain and heat will point out the necessity of again having recourse to it.

Having thus done all that was proposed, namely, decomposed inflammation, the subsequent treatment must be such as is to be preferred, where there is not too much inflammation; and I shall again, once for all, observe, that where there is not occasion to employ remedies for the resolution or dispersion of inflammation, I do not recommend the process of evaporation.

In the gout, rheumatism, ophthalmy, erysipelas, &c. the effects are particularly striking. I have now under my care, a case of violent erysipelas in the face, with both eyes nearly closed up; the alcohol has just been applied, as above directed, and the disease is destroyed.

I must observe, that I have frequently employed vitriolic ether, with the effects that might naturally be expected from so volatile a fluid; but one accident of some moment, and others of less, from the incautious approach of fire, have made me fearful of recommending it; and the more especially as I have always found alcohol fully equal to the end I proposed, and have very commonly chose to lessen its powers, by mixing water with it.

Ether, alcohol, or alcohol and water, may, however, be used with equal safety, by those who have knowledge and care enough to conduct the management of them.

I have now only to add, that, so soon as inflammation is sufficiently repressed, it will be no longer proper to continue the process of evaporation, lest great mischief result from a contrary power to that which produced it, that is, by a too great abstraction of caloric. For it has been already

eady shewn, that a certain relative quantity of caloric is essential to health, and a very different relative quantity to disease; and though each of these admits of considerable latitude, there are, nevertheless, limited boundaries, which, if stepped over on either side, that is, too much or too little caloric, decomposition, and consequently, death must follow.

I would, therefore, urge the necessity of desisting from the process of evaporation so soon as inflammation is sufficiently subdued, at all times denoted by the absence of heat and pain, lest too little caloric be left, or what is the same thing, there be a greater abstraction of it than is consistent with health; and, if carried further, too little caloric be left to prevent decomposition, and thus be productive of gangrene.

A Case of an enormous Tumour in the Cavity of the Abdomen; by Robert Kinglake, M.D.

It has lately occurred to me to see, in the case of a child about three years of age, a large substantial tumour, situated in the epigastric region, and extending its volume throughout the greater part of the cavity of the abdomen. It appeared to originate from the inferior surface of the small lobe of the liver, at least, it was firmly attached to that part. Its weight was upwards of eight pounds, and its texture was for the most part steatomatous; some portions of it were follicular, and others of various and unequal degrees of hardness. The basis of the tumour was so narrow as to admit of its body being easily moved from side to side when pressed on, which afforded an undulating feel not unlike the fluctuating movement of a fluid. Several medical practitioners supposed it to be a clear case of ascital dropsy, and the patient was referred to me for my opinion as to the propriety of discharging the contained fluid by tapping. Nothing but the irretrievably lost state of the child, from the evidently enlarged size of the liver, and its emaciated and dying state, induced me to advise desisting from the operation, lest it should expedite the death of the little sufferer.. In about three days after the child died, when, on opening the cavity of the abdomen, the tumour here described presented itself. It did not appear that there was any accumulation of water in the cavity of the belly.

Had the trocar been plunged into the swelling, as proposed, the deception would have been embarrassing, and would, no doubt, have occasioned additional distress to the patient.

The circumstance of there being a palpable enlargement of the liver, suggested more caution than would otherwise have been observed in treating this case as a mere dropsical affection. When visceral disease unquestionably exists, it deserves to be maturely considered in what proportion a substantial enlargement of organic parts may contribute to that bulk, which should induce suitable doubt and hesitation in concluding it to be absolutely of a fluid nature.

I remember attending a consultation on a case of supposed hydrocele, somewhat similar with respect to the nature of the tumour to the instance here recited, in the Hospital at Gottingen, under the care of Dr. Richter. The swelling was extremely bulky; on examination, it felt uniformly soft and painfully tensive. The question was, whether it was formed by an enlargement of the testicle, or an accumulation of water between its investing coats. The latter opinion predominated, and a trocar was immediately passed into it, when nothing more than a few drops of blood were returned through the canula. The consequent pain became excruciating, and the patient died within eight and forty hours. On examination after death, this ambiguous tumour proved to be a case of sarcocele, and the unsuitable treatment at least hastened the destruction of the patient.

Instances of tumefactions in visceral cavities are perhaps not frequent,* though it is clear that the accretion of

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* Bonetus, in his Sepulchretum sive Anatomia Practica, has indeed recorded, among other somewhat analogous instances, the following more particularly striking examples of this disease.

"Scotus dolebat in hypochondriis, lumbis, &c. Supervenit tumor circa costas nothas sinistri lateris ad os xiphoides prominens: Induruerant pancreas et mesenterium, et in molem supra modum latam, longam et crassam excreverant.

"Matronæ aderat tumor durus ad tactum circa mucronatam cartilaginem, renitens, oblongus: Gibba pars hepatis videbatur tanquam novum corpus simæ superstratum, a qua dependebat tumor oblongus instar pyri.

"Vidi anno 1593, in quadam virgine juvene totum abdomen a Liene occupatum, ipsumque ita exuberantem, ut videtur hæc virgo hydrope laborare: Imo quod gravissimi medici in aspectu cadaveris istius mulieris dissecti, se deceptos fuisse fassi sunt: Erat molli abdomine et suffocata, quamadmodum hydrope laborantibus accidit, de vita discessit." Vide Lib. 3, Tit. de Ventris Tumore.

of new substances, under the stimulus of morbid irritation, may happen to almost any extent compatible with life. If an unusual degree of excitement should invite a proportionate supply of nutritive matter, it may be so applied and arranged as ultimately to produce excrescenecs of as large dimensions as those of the tumour which occurred in the case under consideration. That these diseased secretions often happen is very probable, and they will prove more or less destructive, according to the situation they may occupy, and to the nature of the neighbouring parts on which they may injuriously press. Nor is it easy either to know the existence of such tumours, or if known, effectually to counteract and prevent their progressive augmentation. It is, however, useful to be aware of the possibility of such states of disease, that when they actually occur, they may not be mistaken for others, the nature and appropriate treatment of which may be essentially different.

Taunton, June 12, 1810.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

HAVING observed many useful observations and histories of cases of midwifery in your valuable Journal, I beg leave to transmit you the history of a case, which although it terminated fatally, will tend to shew what has been done on the subject, and point out the necessity of endeavouring to find out some more effectual means in similar cases.

May the 28th. I was desired to visit Mrs. W——— with all possible speed, as she was then in labour. I arrived about eleven o'clock on the forenoon of Monday. I learned she had been in labour since two o'clock on Sunday morning, but it being her first child, and she not supposing herself farther advanced in pregnancy than five months, had not called any midwife sooner, thinking her pains would subside; some of her attendants even persuaded her that it was the colic, and she had obtained some spirituous medicine with a view to relieve herself; but finding this did not answer, she was induced to send for medical aid. I proceeded to examine her, and found the membranes ruptured (which had happened a few hours before) and the

 head

head of a full grown child locked in the pelvis. The external parts were very little dilated and appeared very rigid, which I attributed to its being the first child and to her advanced age (for she was in her thirty-ninth year); the head had advanced in its natural position, and delivery seemed only prevented by the resistance of the external parts. As the pains were frequent and sharp I determined to remain with her a few hours, to ascertain what progress would be made; after which, finding no farther advance nor relaxation of the parts, I took about twelve ounces of blood from her arm and gave her forty drops of opium. This in some measure composed her between the pains, which however continued very violent. In the evening she complained of a frequent desire to make water, when the catheter was introduced and about a quart of water came away. With a view to farther relaxation, an emollient injection was thrown up, and she was ordered a pill of three grains of opium. I remained with her till twelve at night, as her throes continued much the same, and observing no farther advance, and that she was inclined to sleep between the pains, I took my leave for a few hours, desiring to be informed should any change take place.

On Tuesday morning, being still more harrassed with pain, I was called up. I again examined her, and finding that there was no farther advance, and that she was much exhausted by the continuance of the pains, I thought delivery must be attempted; and as the head was so low, the forceps appeared likely to be of service, could they be applied. At this time a continued pain of the abdomen and general soreness called for warm fomentations, which were persisted in for more than an hour, and at the same time a dose of opium was administered. This having somewhat alleviated the pain and restlessness, every thing was deferred for a few hours longer. Examining again, and observing no farther advance of the head, or dilatation, I endeavoured to introduce the extractor, which I accomplished with some difficulty, but was unable to make any progress even with it, as the rigidity of the external parts would scarcely admit of its employment. Fearing therefore lest I should irritate and inflame the parts, I made no farther attempt.

A consultation was now called, when embryotomy was proposed as the only means likely to save the mother, for there was little doubt but the child was dead, from the putrid discharge. As there was no flooding, and as the pains had somewhat subsided, it was judged advisable first

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to give her another large dose (five grains) of opium, and apply fomentations to the external parts, with a view to relax them. This in some degree had the desired effect, but instead of producing rest, the pains appeared to come on with increased vigour, and she continued in this state till towards evening, when they again began to subside. At this time I examined, to ascertain whether those violent pains which had just subsided, had produced any advancement, when I found the head had become somewhat lower, and the parts were more dilated. The only pain which she now felt was a general soreness of the abdomen and a desire to void her urine.

The patient being placed in that position in which we usually draw off the water, I endeavoured to introduce the catheter, but found I could not, on account of the advance of the head. Thinking I could now apply my hands with more effect in this position, I insinuated the fore finger of my right hand along the occiput, and the same of the left over the os frontis, and continuing an undulatory motion for a few minutes, I effected, unexpectedly, the delivery of the head, which was much facilitated by its putrid state, causing it to give way, and admitting the bones to overlap each other. The delivery of the head being thus effected, the body immediately followed in a most putrid state.

Having happily succeeded thus far, our attention was next called to the delivery of the placenta. The hæmorrhage was not very great, but her strength being greatly exhausted, and as there were no pains, it was thought advisable to delay any attempt till she recovered. The umbilical cord was in such a putrid dissolved state that it came away, separating itself from the placenta, on using the gentlest force. She now became quite easy and had several sleeps during the night.

Wednesday morning she appeared tolerably well, and had not experienced any pains since last evening. The fœtor of the discharge was so very great that a fumigating lamp was obliged to be used, notwithstanding which the nurse was seized with a diarrhœa. It was now judged necessary that the placenta should be delivered, lest she should sink under a putrid fever. I therefore lubricated my hand, and began to insinuate my fingers in a conical form, within the os externum, which I found at first slipped into a laceration of the right labium. This being avoided I renewed the attempt, and desiring her to direct her inspiration, and bearing down at intervals, in imitation of pains, I accomplished its introduction. The os tineæ still

remained sufficiently dilated to admit my hand, on passing which, the first thing I perceived was a quantity of coagulated blood, the greater part of which I brought away before I proceeded any farther. I then felt for the cake, which I discovered firmly adhering to the fundus uteri. By inserting my fingers between it and the uterus, and using gentle force, I soon completely separated it; and now grasping it in my hand, I cautiously withdrew it. The hæmorrhage at first was considerable, but soon became moderate. After a few minutes I again introduced my hand, to find whether any pieces had been left detached; but not finding any, I withdrew it, bringing away a considerable quantity of coagulated blood. I judged this precaution the more necessary as the placenta was in so putrid a state that it separated even by its own weight. Thus she was freed of the secundines just twelve hours after the birth of the child, since which the lochial discharge has been but small, and she appears quite easy and composed. She continued in a very favourable state since the delivery of the placenta on Wednesday morning until Friday afternoon, when she was attacked with a violent rigor, succeeded by pain and tension of the lower part of the abdomen. She was now ordered a dose of castor oil, and fomentations to the hypogastric region, with an anodyne in the evening.

Saturday morning much better; has had several stools; pain of the abdomen considerably abated. She has slept pretty well during the night; has had several slight rigors; appetite good, pulse rapid and full, with foul tongue and pain of the head; she was ordered a saline mixture and to abstain from all stimuli. In the evening every symptom recurred with redoubled violence, pulse rapid and strong, breathing difficult, pain excruciating, great restlessness, a continued diarrhœa, and complete obstruction of the lochia. Being thus seized with extensive inflammation, she was ordered to lose six ounces of blood, which although a small quantity, had nearly induced syncope. When she recovered she took a draught of opium and antimonial wine, which gave her some rest, and produced a gentle diaphoresis during the night; when she awoke in the morning she again seemed much revived, drank a cup or two of tea, and only complained of a general soreness and stiffness all over the abdomen, extending down the thighs.

On Sunday evening she became much worse: no appetite, debility so great that she was unable even to turn in bed; countenance became pale; the diarrhœa which had continu-

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ed for two days now ceased. She was ordered to continue her saline mixture, and another sudorific draught was administered. The pulse had now sunk considerably, and putridity had much increased, so that not only the people about her, but even she herself, could scarcely support the fœtor, although the fumigating lamp was still kept at the bed side. I may just observe, that the weather was extremely warm during the whole time of her indisposition. At this time she was much harrassed with frequent retchings and vomitings. A large blister was now applied over the hypogastric region, and the antiseptic plan was now adopted instead of the antiphlogistic. With this view she took a desert spoonful of yeast every two hours, with an anodyne draught. Monday morning she appeared again much better, her retchings and vomiting having entirely subsided. She drank a cup of tea this morning, her appetite being much better than yesterday morning, which I attributed to the salutary effects of the yeast. The blister had not drawn, and it was therefore renewed. She gradually became worse during the day, so that nothing could now be taken but some porter, which was used as her common drink. All her pains having ceased, she appeared quite easy and slept a great deal. I now left her, not thinking she would survive till morning. Calling on Tuesday morning, I found her in a similar state. She was quite sensible at intervals, when the stupor subsided. She continued in a comatose state during the day, and survived till Wednesday morning, retaining her senses to the last, when she calmly passed into the extended arms of death.

Thus terminated this melancholy case on the eleventh day from its commencement. The membranes broke thirty-two hours after labour began; the child was born sixty-eighty hours after, and thirty-six from the rupture of the membranes; placenta delivered twelve hours after the birth of the child, and about fifty-two before the fever commenced.

There is no doubt but the latter part of this case was the disease termed puerperal fever, which became epidemic in London in 1770, and which, I believe, was first particularly treated of as an original disease by Dr. Hulme. He describes it as a disorder which most commonly attacks the patient on the second or third day after delivery, when the belly becomes painful, tense, and sore to the touch, succeeded by rigors, pain in the head, heat, thirst, inquietude, and a quick pulse with difficult respiration and often retching and vomiting. Having inspected several

bodies who died of this disease, he found the omentum and intestines in a highly inflamed state. He therefore very properly attributes the cause to inflammation. In his indications of cure he places much more reliance on cathartics and diaphoretics than venesection, which he cautiously recommends.

I am, &c.

W. HAMILTON.

Ipswich, June 10, 1810.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

SINCE my last communication to you, on the subject of reducing Strangulated Hernia by the Taxis, I have met with five more cases, where gentle and long continued pressure has been practiced. In two of these, I succeeded, but in the rest I failed. These cases, though possessing neither novelty nor interest, much different from those formerly sent you, I have given you in detail. One of them, where the taxis failed, was so uncommonly circumstanced, as to entitle it more particularly to a place in your widely circulated Journal.

Hannah North, a married woman, aged 33 years, but who never had children, applied to me in the year 1806 for a swelling in her right groin, which she first discovered fourteen years before, and had in this interval applied to many surgeons, but without obtaining relief, or any satisfaction, as to its nature or probable consequences. One surgeon, a man of eminence, did actually salivate her for it, and declared her case venereal. Several others, from their manner of treating her, seemed to have similar suspicions. On the other hand, there were some who suspected it to be hernia, but acknowledged there was something mysterious in the case. From anxiety arising from this state of uncertainty, as well as by the imputation cast on her character as a modest woman, (for she then was single) from the supposition of her case being venereal, her life had become extremely unhappy.

On examination of the swelling, I could perceive two distinct tumours bordering on each other, and nearly in the same horizontal plane. The one situated exteriorly was hard, more circumscribed and prominent; the other inte-

riorly

riorly was softer, more diffused, and had a larger basis. This latter tumour frequently lessened when in a recumbent posture, but never entirely disappeared. She also made frequent attempts to reduce it by the taxis, but does not recollect she ever succeeded.

Being satisfied that one of these swellings, at least, was a hernia, notwithstanding the intricacy of the case, I recommended her to apply a steel-truss to this tumour *only*, but I believe this advice she neglected, for on the evening of the 7th of February, 1807, she was seized with symptoms of strangulated hernia. I saw her the following morning, when she complained of sickness and frequent vomiting, constipation of the bowels, and pain in the abdomen, which shot upwards towards the stomach, and downwards towards the vagina. Her belly was neither swelled, nor very painful to the touch; her pulse was 92 and soft, and her tongue a little, though slightly furred. She said the interior swelling in the groin was somewhat painful, had been down some days past, and was now at its largest size.

I made gentle pressure on this tumour for three hours, having previously ordered her five drops of the *tinct. opii* to be given every eight minutes. This remedy, and the manner of giving it in such cases, had been recommended to me by a friend. The aq. litharg. acet. c. was applied in the form of stupes, and pressure the rest of the day and the following night was continued at intervals. By midnight, from mistaking the directions that were given her, she had consumed half an ounce of her drops, but after the first hour of taking them, her vomiting and other distressing symptoms abated.

On the morning of the 9th, the vomiting and pain, attended with hiccough, returned with increased violence. Fifteen grains of the vitriolated magnesia were given her in a dissolved state; but this seemed to increase her sickness. The opium was again had recourse to in the same manner, and with nearly similar effects; and two drachms and a half of it were taken, before it was ordered to be discontinued, and pressure was again applied as before.

The pulse, during all this time, continued to increase in velocity, without any diminution of force; her thirst had likewise increased, and her tongue had become rather more furred; yet, notwithstanding, there did not appear any symptoms of considerable local inflammation. Her belly was neither tense nor much painful, and the tumour bore handling well.

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On the morning of the 10th, she was evidently worse. Her pulse was at 120, but not weak, and her vomiting was frequent and distressing. Her thirst was also considerable, and her belly rather tense and somewhat painful; but still I could not perceive so much of that languor and deprivation of vital energy, which too often characterize and attend this affection. The tobacco-clyster was now administered, (a drachm and a half of tobacco being boiled in a pint of water ten minutes.) This caused considerable sickness, during which, pressure was again made on the tumour for half an hour, but without the desired effect. After the influence of the clyster had subsided, the operation was performed. The incision was begun a little above the upper verge, and carried down below the inferior margin of the tumour. The fascia was next carefully divided; after this, I was surprised on the hernial sac coming in view, to discover a substance attached to the inferior and exterior portion of the sac, thus ♉. This primâ facie appeared to be glandular, and adhered to the sac so firmly, as to render it impossible to ascertain the point at which they were united. This appendage was of an eliptical shape, resembling a moderate sized walnut, both in form and magnitude, and was somewhat flatted on its anterior and posterior parts. An incision was made better than half through it transversely, which discovered its texture, and some blood-vessels apparently enlarged, passing into it, convinced me it was one of the inguinal glands in a diseased state.

Having previously exposed the bottom of the sac by dissection, I desired an assistant to draw it a little exteriorly, and I gently passed the fore-finger of my left hand upwards between the sac and aponeurosis. The adhesions, which were only partial, and extended upwards about three-quarters of an inch, gave way by very little force. Having passed the tip of my finger within *Gimbernat's* ligament, I with the bubonocele-knife divided it upwards, and a little inclining towards the pubes. The incision, as nearly as I could ascertain, was about one-third of an inch in length; and the act of making it, gave me a sense of feeling, as if the knife were dividing something ligamentous. This being accomplished, in a few seconds the sac became less and flaccid, borborigmi ensued, and the patient expressed herself greatly relieved. In order, however, to be certain that the hernia had entered the abdomen, I pushed up the sac as far through the aperture as possible, without violence, but found the gland, as well as the adhesions of the

the sac to the contiguous parts, considerably to impede my attempts. On handling the sac, I thought it much thickened; and I now made some attempts to separate it from the gland with my fingers, but found this also impracticable.

Being now satisfied the object of the operation was attained, the integuments were brought together over the gland and sac, by three stitches of the interrupted suture, assisted by straps of adhesive plaister, so as completely to unite the wound by the first intention. Nothing further occurred relating to the cure, of any importance. The wound healed kindly, and was attended with very little pain; and in twelve days was completely cicatrized.

The pulse, at the time of the operation, was at 122; an hour after, at 108; and the day following, at 96. In short, she rapidly regained her health.

It may be a matter of surprise, that the gland was not removed at the time of the operation, on account of its diseased state; but as it had preserved its texture, and exhibited no marks of disease, save enlargement; and as extirpation could not have been attempted, without probably leaving a part of it behind, or otherwise wounding the sac, I therefore suffered it to remain, being persuaded the operation would effect some change towards its annihilation or dispersion. This idea was eventually realised; for on the 15th of September, 1808, I was called to this patient, who complained of great pain in the hernial sac, but which was empty. Her pulse was at 103; she was thirsty; and her tongue a little furred. She was also excessively sick, but had not vomited, yet had retched a few times. Half a drachm of the vitriolated magnesia, dissolved in a table-spoonful of cold water, was ordered her every hour. In ten minutes after taking the first dose, her pain and sickness ceased; and after repeating the medicine a few times, it operated, and she ailed no more. On examining the groin, I could perceive no remains of the gland. She informed me, it vanished in a few weeks after the operation; and that the hernia now descended and receded, accordingly as it was favoured by the position of the body, more readily than formerly. From which time, to the present, she has worn a steel-truss, and continued in good health.

This case affords no *data* for determining the effects of pressure, for it is plain, from the frequent and ineffectual attempts of the patient formerly, at different times, to return the hernia, and the circumstance of the sac adhering

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to the inguinal gland, which prevented it being pushed up towards the stricture, that no pressure, on the principle of pressure, formerly recommended, could have much effect.

I have long been impressed with an idea, that under some circumstances, the hernial contents might with propriety be returned into the abdomen, without a division of the sac, being sensible of the advantages that would accrue from, and the inconveniences that would be obviated by, such a practice. Under this conviction, I have ventured to deviate from general custom; and although I have great, and almost general authority against me, yet the few observations I shall adduce on this practice, will, I trust, not only apologize for my conduct, but may call forth the more experienced part of the profession to a further discussion of this subject.

Two ostensible advantages apparently result from a division of the hernial sac. The first is, the greater certainty of removing the stricture; the second is the opportunity afforded for inspecting the hernial contents. As to the division of the sac, for the first intention, I believe it may frequently be obviated by the stricture being otherwise removed; and as the method of dividing the sac is counterbalanced by such important disadvantages, which are nothing less than rescuing the patient from one danger and exposing him to a second, any method that will effect the one object and prevent the other, is a desideratum in Surgery of no small moment.

What has confirmed modern surgeons in their idea of the necessity of uniformly dividing the sac, seems to have arisen from a contrary practice, adopted many years ago by some eminent surgeons, but which grew into disrepute, principally by their not attending strictly to the circumstance of ascertaining whether the stricture was overcome. They imagined, when the tendon of the oblique muscle was divided, that the stricture was of course removed. They found, however, on examining into the cause of the miscarriage, which in some instances happened, that the neck of the hernial sac in general formed the stricture, and was the source of the mischief. Hence, the old method of dividing the sac was reverted to promiscuously, without adopting or retaining any modification of the new practice; the utility of which, had, in many instances, been conspicuously evident.

By the non-division of the sac, the patient is certainly exposed to some risk from the neck of the sac forming the stricture, as has sometimes been the case; and it is pretty certain,

certain, that in a great majority of the patients, who have died after the operation, where the sac has not been opened, it has *been* from this cause; to ascertain and remedy which, I believe in most cases, will not be difficult. After dividing the integuments, and properly exposing the sac, I would desire an assistant gently to pinch up the sac with his finger and thumb, in order to prevent it slipping into the abdomen; a circumstance that might possibly happen, but as the sac most generally adheres to the surrounding parts, this part of the operation will mostly be rendered unnecessary. Immediately after this, I would divide the supposed stricture; namely, the tendon of the oblique muscle in the inguinal, and Gimbernat's ligament in the femoral hernia. This being effected, the protruded parts, especially if the patient be laid in a posture favourable for reduction, will in most instances readily recede. The attainment of this object is generally known by a diminution of the pain and tumour, attended frequently with borborigmi, and succeeded by an alteration in the countenance, and particularly a sanguine manner in which the patient seldom fails to express his relief; but if, on the other hand, after waiting a while, any doubts of the stricture being removed still remain, it will be then necessary to open the sac. The few solitary instances we meet with in surgical authors, where some part of the hernial contents form the stricture, can have little weight towards pointing out the necessity of opening the sac, when set in opposition to the disadvantages resulting from that part of the operation.

Hence, if on dividing the tendon, every symptom of strangulation cease, together with a reduction of the hernia, and the sac at the same time remains unreduced, we may rest assured that the stricture is not in the neck of the sac. If the circumstance of the hernial contents forming the stricture, were not a very rare occurrence, a mode of operation, perhaps, preferable to the above, might be suggested; this scheme, though not founded on experience, is the result of some reflection. The method which I allude to for determining whether the stricture be in the protruded parts, is as follows. After the integuments in bubonocele are divided, and the sac properly denuded, the patient's body is to be elevated into a half-erect posture, so that the axis of his trunk may form an angle of about forty-five degrees with the table on which he is laid. By this position, the hernia will be prevented from spontaneously ascending. The tendon is next to be divided, and in such a manner, if the cause of the mischief, as to leave no

no doubt of the stricture being removed. If all the symptoms of strangulation should now cease, it is conclusive, that the stricture is in the tendon of the oblique-muscle, and the hernia is to be returned into the abdomen, as before described. If, however, after waiting a while, any doubts of the stricture being removed are still entertained, a fair opportunity offers for opening the sac, for the purpose of discovering the stricture. The advantages which this method seems to possess, are sufficiently obvious. First, it ascertains whether the stricture is in the tendon, or in the sac, or some part of its contents; if in the former, then the hernia may be returned, without a division of the sac, where the object is to remove stricture only; if in the latter, then the real necessity for opening the sac is indicated.

In irreducible hernia, in an incarcerated state, by the non-division of the sac, we have fewer means in the operation of ascertaining the removal of the stricture, than when the parts are capable of being returned into the abdomen. The feelings of the patient, and a suspension of the symptoms, are our principal criteria. Notwithstanding these scanty means of discrimination, before we expose our patient to the consequences of dividing the peritoneum, we ought to wait a while, as before mentioned in reducible hernia, after the division of the integuments and tendon if the circumstances of the case will admit it, and if a total cessation of all the unpleasant symptoms should ensue, for, we will suppose, the space of an hour, we should not hesitate to unite the wound by the first intention.

This practice of waiting may appear trifling to the patient or bye-standers; but to the humane surgeon, who is fully impressed with the consequences dependent on his conduct, and who wishes to avoid incurring danger, rather than try the experiment of overcoming it, this practice, I hope, will have its due weight. As this method also can only be advisable, when our object is to remove stricture, and as this must often succeed, without exposing the patient to those consequences arising from a division of the sac, such an attempt, guided by caution and judgment, ought in my opinion to precede this most serious part of the operation.

The reasons for not opening the sac, in reducible, apply equally or with greater force to irreducible hernia. For in the latter, as soon as the stricture is removed, we are to consider the prolapsed parts, either in a state of inflammation

tion or gangrene. If in the former, then the advantages will be greatly in favour of not opening the sac, for reasons that are obvious. If, on the other hand, mortification exist within the sac, it is in general confined to parts below the ring; and the enveloping covering will impart such warmth to the diseased hernia, as cannot fail not only to arrest the progress of mortification, but to expedite the process of separating the gangrenous from the sound parts. Hence, as this business cannot be effected without suppuration, a sanious matter will form, and accumulate in the sac, which, if not evacuated by an artificial opening, and left to nature, will be discharged by a spontaneous rupturing, attended with considerable loss of substance. On the contrary, if the practice of opening the sac be adopted, at the time of the operation, and the morbid part of its contents be removed by the knife, the consequences are frequently fatal; and, if even they are only in a state of inflammation, from their being previously thickened and diseased, any new stimulus applied, or additional violence done to them, must cause the subsequent inflammation to be alarmingly great.

The advantages necessarily resulting from this unusual mode of operation, as above recommended, are such as have perhaps not been duly appreciated. Air, when admitted into the abdomen, has been long considered as deleterious; and the division of the sac, in a state of irritation, which is the peritoneum itself, must in conjunction with this circumstance, be sufficient greatly to aggravate all the distressing symptoms which existed at the time of the operation. Whoever has seen the difference betwixt wounds in the abdominal covering, where the peritoneum has been divided, and otherwise, will be satisfied, this membrane cannot be injured without an increase of subsequent inflammation; but the most convincing of all arguments will be, the contrast between these cases, where the operation has been performed in the usual way, and those where the taxis has succeeded. This difference in their result is so striking, as may draw the attention of surgeons, at some future period, to a more able consideration of this subject. Mr. Wilmer gives us a number of apparently desperate cases, where the hernia was reduced without the assistance of the knife, and the patients ailed no more; and he also gives us other cases, where the operation of dividing the sac was performed in the usual way, and a great proportion of the patients died. Mr. Hey seems to have been no more successful in his practice; yet both these gentlemen, as well

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as others, have attempted to reduce the hernia, previous to the use of the knife, therefore their object in dividing the sac, could not be for the purpose of inspecting the hernial contents, but for removing stricture only; and as this may generally be effected without a division of the sac, it follows, that I was justified in performing the operation, as above described.

From the preceding premises, as to the propriety for and against opening the sac, the following inferences may be deduced.

1st. That a great proportion of those cases, where the sac has been opened, and the hernial contents have been in a diseased state, have terminated unfavourably.

2dly. That a great proportion of those cases, where the stricture has been removed, without a division of the sac, have done well.

3dly. That in all those cases, where the taxis is performed, the surgeon is justified in returning the hernia by any other method, save that of opening the sac; because the taxis implies, that the parts are in a fit state for reduction.

4thly. It also follows, that if the method above described, of returning the hernia, without opening the sac, be unjustifiable, the operation of the taxis immediately preceding it, is unwarrantable.

5thly. That if the state of the case require the hernial parts to be inspected, any attempts at reduction, immediately preceding the operation, will be inadmissible.

6thly. That in most instances, where the operation is performed, without dividing the sac, and symptoms of stricture still remain, the seat of the mischief is generally in the neck of the hernial sac, which is to be opened accordingly.

As to the theory of returning the sac unopened, I wish to be understood in a qualified sense; and in order to prevent misconception, I will be as brief and explicit as possible.

In all cases, where the surgeon on due deliberation, judges the hernial contents to be in a fit state for reduction, and performs the taxis accordingly; in such cases I would not hesitate to adopt the practice, under the precaution and rules before described; and it is on this ground only, I wish to advise or recommend it. Again, if the taxis be right, and it fail, this method cannot be wrong; or, in other words, if it be right to perform the taxis, can it be wrong to return the hernia, with the sac unopened, by an operation? Where is the difference? In the

the one instance, the stricture is overcome by the hand; in the other, it is removed by the knife. Are not the symptoms, peculiar to reduction, common to both methods? Are not these common symptoms as conclusive in one case as the other? What surgeon will hesitate to speak decidedly, when he succeeds by the taxis, as to the certainty of the hernia being reduced? What reasons, therefore, will he have to doubt the return of the hernia by the other method? Cannot he ascertain the reduction in one instance as well as the other.

(To be Continued.)

On the Danger of Curing certain Diseases.

(From the French of Mons. Raymond, nouvelle edition par M Giraudy.)

ONE of the most dangerous errors in medicine is the desire of curing all diseases. The methods which are employed for this purpose oftentimes are inimical to nature, interrupt her efforts, and occasion accidents more or less unfortunate. If we obtain a cure, it is frequently a very imperfect one, we only shift the disease or conceal it for a time; for daily experience shows that this pretended cure lays the foundation of some other disease often more grievous than the first, or at least weakens the constitution very much and renders the health more delicate, and thus shortens the duration of life. Forty-eight years practice, mostly in a large and populous city, have at least convinced me, that it is often better to abstain from all remedies, than to incur the evils which arise from the removal of an inconvenience at once salutary and easy to be borne. I mean to show the bad effects which are produced by the humours either from their noxious qualities, or from their quantity, when they are retained, or driven inwards; with what circumspection young physicians ought to conduct themselves towards those who consult them for their ailments; and how the public should be on their guard not to listen to the multitude of ignorant people, who, destitute of skill and experience, have the impudence and effrontery to prescribe medicines for all their complaints. What sagacity and knowledge of diseases ought he not to possess, who undertakes to determine whether any one disease or symptom is useful or prejudicial, and consequently, whether he ought to remove it

it or suffer it to continue? And if the most able physician is often at a loss, as to what course to pursue; in doubt as to the time, the means, and the choice of remedies; what can we think of a person who possesses neither science nor practical experience of diseases?

It was perhaps with the same impression, that an English physician* published, about the end of the last century, his treatise, entitled *Ars curandi morbos expectatione*; that Celsus said, many diseases are cured by diet and by rest; that almost all celebrated physicians acknowledge, oftentimes the best remedy is doing nothing, and that polypharmacy or a multiplicity of remedies is the resource of ignorance, or a sign of little faith in the physician, or of the weakness of his judgment.

I should have thought myself wanting to the public, which the laws of religion no less than those of society command us to love, if I had withheld from them these reflections, which are founded upon my own observations, faithfully related, and upon those of some authors highly worthy of credit. I only beg of my readers to peruse them without prejudice, and to recollect what was said by Richard Morton,† that of all authors, none had enriched the science of medicine except those practitioners who had faithfully and accurately given their own observations, and had detailed from nature, the history and progress of each disease.

Article 1.—On Habitual Sweats.

It is not my intention to enter into a consideration of all the different kinds of sweat, I shall confine myself solely to that which is habitual and advantageous, although perhaps inconvenient, and endeavour to show, that we ought not to check it or interrupt it, especially by external applications; but at most, if it becomes insupportable, to treat it by proper regimen and simple remedies; for upon its being suddenly stopped, either spontaneously or by art, it is frequently followed by colics, diarrhœa, or violent dysentery; sometimes by fevers, cough, epilepsy, hæmorrhage, &c. These habitual sweats are not commonly universal; we find some individuals who sweat only in their feet, others who have moist hands, others in whom it is profuse on the head or in the arm pits. In some this fluid has neither smell nor colour, in others it is strong and warm; in some it is viscid, greasy, and often cold; every one however is eager for remedies to rid him of the inconvenience, which

* Gideon Harvey.

† In Prefation. Physiolog.

which being in fact an effort of nature to remove something noxious or superfluous from the system, often resist all the means we can employ for its removal; or if by chance any one attains the object of his wishes, he dearly pays for his weakness and imprudence.

It would be tedious to relate every thing that has been observed and written by our celebrated practitioners, on the alarming consequences of suppressing these sweats, and upon the care we should even take to encourage them; I shall only give what I myself have observed upon this matter.

A religieuse, who had in her infancy and youth been subject to a humour in her eyes and eye-lids, attended with a flow of tears, with redness and sometimes with inflammation, was relieved of these complaints at the age of puberty; and at the usual time of the appearance of the menses, her feet and legs became subject to profuse sweating of a disagreeable odour, and which was increased in spring and summer. As long as she bore this inconvenience with patience she enjoyed good health, but being at length desirous to get rid of it, and no longer to incommode thereby her companions in the church and in the refectory, she by the advice of some old woman washed her feet and legs with a very astringent aluminous lotion, which effectually prevented the sweats: but a short time after she was seized with epileptic fits, which continued frequently to recur during three years, and which at length gave way to no other remedy than change of scene; the glands of her neck and in the axilla then became large and swelled, the body was covered with pimples and pustules; at last a scrophulous pulmonary phthisis succeeded, accompanied with a cough and purulent expectoration of a yellowish and greenish colour and a slow hectic fever, thus terminating her days at the age of forty years, the whole of which, notwithstanding the regularity of her menses, she had passed in ill health, except the time the sweating continued.

I must not pass over what I experienced in myself during the plague which raged in Marseilles in 1720. As soon as that cruel malady appeared, I perceived my arm-pits, contrary to custom, very hot and moist, and sometimes I experienced heats and smarting scarcely supportable; this, which was new to me, continued during the whole time the plague prevailed, and left me when that had entirely ceased, that is to say, in the spring of 1721.

The plague re-appeared in the spring of the following year, and I also experienced a return of the heats and

sweating in the axillæ, but they disappeared with the entire extinction of the plague, of which no trace was left by the autumn of the same year.

Although during this state, and at the two different times mentioned, I was employed in the care of those affected with the plague, of whom I daily saw a great number, I can say I never enjoyed better health, I even became more jolly than before. I think, however, I should infallibly have sunk under the hardships, the vexation, and the risques to which I was exposed, if Divine Providence, to whom I owe eternal gratitude, had not subjected me to these salutary sweats during this dreadful period.

Notwithstanding I have just now said, that these habitual sweats being seldom universal, but generally partial, do not derange any function of the body, and are salutary; that the health and even life are endangered whenever they are suppressed either by nature or art; it may not be amiss to point out the remedies that should be used by those who are much annoyed by the smell of this discharge, which is sometimes almost insupportable to themselves as well as to others, and obliges them to seclude themselves from company, and even to abandon their trade or profession, and who are willing to attempt without risk, the removal of such an unwelcome malady.

As to those whose sweats are neither very excessive nor fœtid, and who are not weakened thereby, I can give them no other advice, than to bear them with patience, to keep themselves clean and neat, to wash their skin often, and especially the parts affected, with warm water, and a little orange flower water, or with a weak decoction of some aromatic herbs, and to avoid all excess of eating, to refrain from salted meats and spirituous liquors; to preserve themselves from violent passions and immoderate fatigue; in a word, to do nothing which will increase the elasticity of the fibre or the circulation of the fluids.

For those whose sweats are cold, clammy, and of a bad odour, I know nothing more suitable, or that has succeeded better with me than a generous diet, roasted meats rather than boiled, or ragouts, or if the latter are preferred, to season them well with spices, shalots, saffron, onions, and even a small quantity of garlic, and to allow them old wine of a good vintage.

In addition to this regimen, we must employ purgatives and bitter decoctions; a drink prepared from China root or sarsaparilla, and viper broth, and also give the filings of iron or steel, which should be continued a length of time

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at proper intervals; if to these remedies we join cleanliness, external washing with decoctions of aromatic herbs, and frequent exercise, it is hardly possible but that these sweats, from whatever parts they proceed, or to what degree soever of clamminess they may have arrived, should not be changed in their nature, and their bad qualities be either totally destroyed or greatly corrected.

If however all these means are ineffectual, and the sweats proceed from the head or the arm-pits, we should open an issue in the nape of the neck or in the arm; but if the seat of the complaint should be the groin, the thighs, or the feet, it will be better to open it in the leg, This is our last resource for overcoming this disease, when it is insuportable to the patient and his friends, when it has resisted all other means which have been thought of. Zacutus Lusitanus * has found it effectual in similar cases. But if the issue is of no service, we must always avoid every thing repellent, as the cold bath and all astringent lotions, ointments, &c. lest we hurry our patient into the grave.

It may be said that the issue is as troublesome and disgusting as the sweats, and that it often diffuses a smell as disagreeable as that which it is designed to remove; it is true the issue must be kept open a long time, and must be often dressed; but the advantage derived from it more than counter-balances the care and attention it requires, and if it occasionally offends by its smell, this may be obviated by dressing it twice in the day, by covering it with a plaster, or linen, or perfumed silk, and to keep it open, we should not use peas, but little balls made of wax, of orris root, or yellow sanders.

Article 2.—Itchings.

This disease may be reduced into four classes, according to its causes and origin.

The first is critical; it arises from an internal cause, and is the consequence or termination of some acute or chronic disease, as when it terminates a continued or intermittent fever, of which it indicates the finish and cure.

The second arises spontaneously, appearing of itself, without any preceding indisposition; this species also is owing to some internal cause, and is generally the effect of an accumulation of gross humours, formed by degrees in

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* Observat. 71. libr. 3, praxis admirand.

in the system, which, quitting the internal parts, are carried by transpiration under the cuticle, producing there a kind of itch, which chiefly attacks those who feed upon gross aliments, such as salted meats, of beef or pork, leguminous vegetables, green sour fruits, &c.; those who have drunk much wine loaded with tartar, those who have respired the sea air, and who have not been careful to wash their bodies, and keep their skin clean. This species commonly affects those who have taken long voyages.

The third is symptomatic, and owes its origin also to an internal cause; it is the effect of and usually accompanies some other disease, as the jaundice, the venereal disease, &c. Nothing is more common than to see persons affected with jaundice, having their skin not only yellow, but rough and studded with risings or small pimples, accompanied with a great itching; they also sometimes have their transpiration and sweat yellow as well as their urine. It must then be the bile retaining its colour, united to the matter of transpiration, which it renders more gross, causing it to be stopped in the pores of the skin; these become filled with it, and are soon enlarged and opened, the same as the scarf skin where the pimples show themselves, as also the pustules and the itching, all symptoms essential to the itch.

It will be the same with the lues, if the venereal virus is floating in the blood, or, to speak more correctly, is mixed with the lymph, it is carried by transpiration to the cutaneous pores; they are overloaded with it, and there will follow pimples, pustules and itching, consequently a venereal itch, referable to the cause which produced it.

The fourth and last species of itch is that which is produced from without, and is entirely owing to an external cause, such as contagion, frequent contact or cohabitation with a person affected with the disease. In this case, the moisture, the scurf, the scales, and every thing that falls from the skin of a person affected, may easily adhere to the skin of a sound person, and there produce erosions, pustules, and itchings, similar to those of the person first affected; moreover, as it is only in the skin it makes its appearance, and if what comes from the skin of the affected person has shut up the pores of the sound skin, and has impeded the transpiration through it, and if by a necessary consequence those pores are filled and obstructed by the infection, we may readily conceive, that in this way the itch may easily be transferred and communicated from

from a person infected with it to another whose skin is healthy.*

I shall not deal in mere possibilities, or vain speculations, but shall give practical facts, as they have been observed by myself and some other of our authors.

(To be continued.)

Perhaps one of the best means of judging of the state of medical science and medical practice in a country, is a perusal of the works most in repute, and generally esteemed in that country; with this view we shall occasionally give an extract from some new French book, that our readers may see how far our neighbours on the continent precede or are behind us in this respect. EDIT.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

I Am a friend to true and candid criticism, and therefore am obliged to your correspondent A. B. for having offered some remarks upon my letter on Superfœtation; not that I think he has exactly, in all respects, treated it in the manner I could have wished, because the legitimate and *sole* end of criticism is truth; therefore, whenever I find a person turning aside from the points on which the argument depends, to ridicule unimportant deviations from logical precision, I begin to fear, that, in his heart, there is some hankering after victory, to sully the purity of his endeavours in the cause of scientific truth. I need scarcely add my thoughts on the observations of A. B. on the lapsus which

* The itch, properly so called, is to be regarded as a cutaneous eruption *sui generis*, and is characterized by the following symptoms. An eruption of pimples, on the top of each of which there appears some lymph, at first transparent, at length becoming dry; it is accompanied by violent itching, increased in the evening and at night; these pimples are more numerous in the fingers, wrists, loins, hams, and groins, than in other parts of the body. The itch is contagious. If it is very violent, continued watching, agitation of the whole body, extreme itching, want of appetite, hemicrania, and slow fever are the consequence; the system in general becomes infected, a dry cough and wasting of flesh succeed. It is sometimes combined with syphilitic, or herpetic virus, &c.; it may be repelled inwardly, and falling upon some important organ, may there produce chronic diseases of the worst description, as asthma, phthisis pulmonalis, abscess of the liver, hysteria, mania, &c.

which had glided into the first part of my last letter; especially too, in a case like the present, which, when examined with the context, may be defended as being no lapsus at all, because it merely resolves itself into this question, have "the majority of practitioners attentively considered the anatomy of the gravid uterus?" It may be my opinion that they have not.

I consider it extremely unfortunate that I was ignorant of the papers upon the same subject, which appeared in Nos. 125 and 128, of your Journal. From my seeing the Medical Journal only through the uncertain medium of a Book Society, those papers had and still have escaped my notice, so that I am still as ignorant of their contents as I was before the publication of my last letter.*

I now proceed to examine A. B's objections to what he has *made* me, somewhat ostentatiously, give the title of a new doctrine of superfœtation. A. B. does not in any place deny my conclusions, except, indeed, indirectly in one instance, where he shows a considerable want of knowledge in the practice of midwifry,† but levels his shafts at the arguments which are brought forward to prove them; and at these merely upon the principle, that I consider them the *only* cause which prevents superfœtation from taking place. That this is my opinion cannot be deduced from my premises; because, although the road to the ovarium being stopped, may be considered as a cause adequate to the effect, yet, that is no reason why there should not be other causes acting, and acting more forcibly, for the same end. The truth is, that in proving an opinion, you can only make use of tangible arguments. I could say, as well as A. B. or Dr. Haighton, that after impregnation has taken place, the organs of generation become changed in their powers and functions, which disqualifies them from further impregnation as long as gestation is going on; but this is mere opinion; we are unable to demonstrate these changes, and therefore it becomes an inadmissible argument on such an occasion as the present. A. B. indeed remarks, that they are made clear by the experiments of Dr. Haighton.

* In this place A. B. has given an unqualified assertion, as a quotation from my letter, which upon re-perusal he will find is qualified with the word perhaps ——

† He here states, that the plug which stops up the os uteri during gestation might become the cause of rupture of the uterus, when he ought to have known that the first act of labour is the expulsion of this plug.

Haighton. What these experiments are he does not mention; and as I have been unfortunately debarred the opportunity of gaining information from the learning of that gentleman, I should feel much gratified if A. B. would favour me with the proofs upon which he grounds that opinion; for, as circumstances at present strike my mind, there seem to be several facts which are in opposition to it. Perhaps, however, this seeming contradiction may only depend upon the different times at which we fix the termination of impregnation. If Dr. Haighton considers the process of impregnation not to have terminated till the fœtus has descended into the uterus, such an opinion can never be correct, because abundance of cases have happened in which the ovum never has reached the uterus at all, yet still is nourished; but if, on the contrary, he thinks it takes place immediately upon the liberation of the impregnated ovum from the ovarium (and I conceive that no intermediate time can be allowed); then the fact related in my letter from Dr. Hamilton, of bitches bearing puppies of different breed and others of the same kind, seem to stand in direct opposition to the opinion above mentioned. Of this kind, of course, is the fact of the American black woman, mentioned in my letter, which A. B. rather ungraciously doubts being a fact, upon the mere inaccuracy of Mr. Fawell or some other person, who happened to say, that the children were one black and the other *white*, which the commonest understanding must know could not possibly have been the case, as one child must have been a black and the other a *mulatto*. I cannot help finding fault also with A. B. for *making* me say, that, in my opinion, the fright received by the woman was the cause of the death of the child; this he will find was no opinion of *mine* in the slightest degree.

I should like A. B. to be a little more exact in his examination of my expressions, especially after having begun his remarks by finding fault with their accuracy.

I shall with great pleasure see a more satisfactory answer from A. B. for at present I must own, that I do not think he has established any thing of consequence against my positions.

I am, &c.

H.

July 8, 1810.

To

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

AS Ophthalmia, commonly called Egyptian Ophthalmia, has been so prevalent in the army these last six or seven years, and has of late made its appearance in private life, I conceive it a duty to communicate any thing that appears to lead to a more successful treatment of it. The generally received opinion is, that it was communicated by contagion from those troops who suffered so generally in Egypt, and since their return. A disease so novel and destructive, has naturally created much anxiety and investigation into its sources and mode of treatment; and the profession are indebted to Dr. Vetch for the able and accurate history he has given of the disease, as it appeared in the 52d and other regiments.

That it is contagious, Dr. V. considers certain, from the time of its appearance and other causes he assigns; but he observes, "the situation of Hythe barracks, the hot season of the year at which the disease made its appearance, from the exercise which the men daily underwent, exposed to the reflexion of the sun's rays, from the shingle and the great quantity of fine sand, which a long prevalence of blowing weather carried from it, a ready explanation of the origin of the disease seemed to present itself; yet as other regiments similarly situated and exposed, were not affected, these causes did not appear satisfactory. In another passage, he also mentions the disease prevailing chiefly in the neighbourhood of the Delta; and that moisture does contribute to the aggravation of it, as most strikingly observed in the increased and diminished severity of the symptoms, on the removal of the regiment from Riding-Street barracks to Maidstone, and its return to the former quarters, which are situated in a most extensive Marsh.

I do not mean to deny its contagious character, but I wish to draw the attention of others to the above causes, that it may be farther ascertained whether they can give rise to the disease, or excite it into action, or aggravate it in rendering the symptoms more violent, or in changing the character of the disease from the simple continued inflammation of the eye, to the more formidable intermittent form it has of late assumed. For in no one instance that has come under my observation, was I able to trace any contagious source.

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The first time the disease came under my treatment was last summer, when Surgeon to the Cambridge Regiment of Militia; the greater number of cases were slight, and gave way to the usual simple treatment; other cases more active required general blood-letting, &c.; no doubt was entertained but it arose from the prevalent windy weather blowing up much fine sand and dust, accompanied with heat and reflexion of the sun's rays. After thirty or forty men had been attacked, the regiment then stationed at Yarmouth, removed to Chelmsford; the weather continued unusually hot and dry, and the men were daily attacked; after two or three weeks, the weather became cool, and much rain fell, (June) and about this time, two or three that had been attacked, and were considered convalescent, relapsed, and assumed the intermitting form with all its formidable train of symptoms. Bleeding largely, with purgatives, &c. and the antiphlogistic regimen, were employed without arresting the progress of the disease; in one case ulceration took place, and the other ruptured without ulcerating. I also had an opportunity of seeing several unfortunate cases at the Ophthalmia Depôt, Danbury barracks.

A short time after this, (in July) I was in town, and called on the late eminent Mr. Saunders, and related the symptoms and mode of treatment pursued; he said he had seen the 52d when labouring under the disease, and regretted circumstances prevented him staying with them so long as he wished; a great many cases had, however, since come under his treatment, and he was of opinion, bleeding, though sometimes necessary, was in many subjects attacked with the disease inadmissible; and mentioned that he had given bark in some strong marked cases with immediate relief from the paroxysm, &c.

On my return to the regiment, the two men in whom rupture and ulceration had taken place, continuing subjec[t] to the paroxysm, I immediately ordered pulv. cort. per ʒ 2da. q. q. hor. sum.; and with the exception of one sligh[t] paroxysm, they had no farther return. I have since ha[d] several cases in private practice, which I will merely gi[ve] an outline of; Dr. V. having so fully described the symp[-] toms and treatment, it will be unnecessary to detail the[m].

T. M. Esq. was attacked on the 17th of May with slight inflammation of the right eye, and had two irregular pa- roxysms of pain, after which it became better, and the le[ft] eye was attacked; he applied a weak solution of ceru[s.] acetat.

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20th and 21st. He had paroxysms in the evening.

22d. I was requested to put some leeches on at 4 P. M. the eye was then free from pain, but extremely vascular, and discharged tears copiously, with some purulency, and considerable sense of heat; the leeches afforded no relief, and the paroxysm occurred with increased violence at six o'clock; about eight I saw him, and ordered a draught with tinct. opii, gt. 40; this rather mitigated the pain, which continued six hours, when it subsided, leaving a great sense of heat, and constant discharge of tears, scalding the cheeks as they passed over.

23d. Ten o'clock A. M. has had occasionally a throbbing pain since six, and now feels sense of heat and enlargement, &c. as yesterday. Capt. haust. aper. cont. lot. saturn. Requested to be sent for next paroxysm occurring.

24th. Four o'clock A. M. paroxysm had been on an hour and a half. V. S. ℥xxx.; syncope took place, and he was relieved from all symptoms as long as it continued, but on recovering, sense of heat returned. Appl. emp. canthar. inter. scapul. Three o'clock P. M. another paroxysm has occurred, and been on him two hours. Capt. tinct. opii, gt. 40. A short time after, the pain subsided; but early in the morning (one o'clock) of the 25th, it occurred more severe, and lasted six hours, leaving an intolerable sense of heat, tears, and matter flowing copiously, from which, local applications afforded no relief. I now resolved to purchase a clear intermission by another bleeding, and throw in the bark. I accordingly opened a vein, and after taking about twenty-four ounces of blood, I wished to have desisted, if the pulse had fallen, or he felt the least faint; but these did not take place till near forty ounces were drawn; he then became faint, and soon afterwards extremely sick and low; coffee and a little wine were given, after which he fell asleep, and in five hours from drawing the blood, the sense of heat slightly returned. I immediately commenced giving decoct. cort. ℥ij. c. extr. cort. ʒß. every hour, and continued it for twelve hours, when it was given every two hours.

In the evening of the 26th, a little febrile heat was observed; and as it increased, the bark was desisted from, and a draught with aq. amm. acet. c. spt. æth. nitr. substituted during the night, and in the morning he recommenced the decoct. c. extr. every two hours.

28th. Free from pain and sense of heat; discharge of tears and pus less. Cont. decoct. cort. c. extr. 4ta. q. q. hor. After this time the eyes became gradually less vascular,

cular, discharge of tears and matter decreased, and the patient gradually, though slowly, recovered perfect vision.

Case II.—Mr. T. Jun. Opened the bowels, applied leeches, and used saturnine lotion without relief; the paroxysm lasted four hours; on its recurrence, gave a draught with tinct. opii æther a. gt. xxx; this relieved the pain, after which I commenced giving pulv. cort. p. ℈ij. 2da. q. q. hor. and he had no farther return.

Case III.—Mrs. M's paroxysms had occurred three times, lasting two hours; the bark was here given as in the former case, and she had only one slight attack after.

Case IV.—Mary Papworth had been five weeks subject to slight paroxysms, lasting two hours, and complete opacity of the left eye had taken place. The bark was here given with immediate relief, absorption took place, and she has perfectly recovered the sight.

In the above cases, there was no constitutional affection discoverable by any symptom, the disease was confined entirely to the eyes.

The pain, as described by the patients, darts, throbs, and shoots with a sense of weight and fulness.

In the first case, I regretted bleeding a second time very much, but the symptoms were so formidable and urgent, I was fearful of a rupture taking place; and I did not think myself justified in giving the bark till a clear intermission took place. Bleeding, from the observation I have made, will arrest the symptoms, but does not prevent the recurrence; indeed, I am disposed to think the succeeding ones have been more violent, in consequence of the large depletions, and I shall be induced in any future case (where opium will not succeed) to try the effects of large nauseating doses of antimony in gaining an intermission, afterwards giving the bark.

I might have enumerated more fully the symptoms and the appearances of the eyes under the disease, but as I could have made no improvement or addition to the accounts published, I did not think it necessary. I only wish to draw the attention of medical men, more particularly in the army, to the administration of bark in the intermittent form of ophthalmia, in the full confidence of its success.

I am, &c.

HENRY HEADLY.

Cambridge, July 14, 1810.

Case

Case of Tumour on the Tongue, with cursory Observations on the Use of the Carbonate of Iron in Carcinomatous and Phagedenic Ulcerations. By R. Hall, M. D. London.

La vraie experience est l'experience generale, qui resulte des observations particuliéres de tous les lieux et de tous les temps.

THE want of a clear and plain definition of cancer has frequently been a subject of much regret among the practitioners of the healing art. Many writers have accordingly endeavoured to supply this desideratum, but hitherto with little success; for though we find various definitions given of it in nosological systems, all of them, it may be affirmed, are imperfect and insufficient for the purpose of accurate distinction; since they still leave us in ignorance of what constitutes its essence, or essential character. Hence, scarcely any term has been more vaguely used than that of cancer; some applying it indiscriminately to all inveterate and corroding ulcers, in whatever part situated, which resist the common methods of cure; while others, on the contrary, confine it merely to indurated, and unequally enlarged glands terminating in ulceration. If, however, through our ignorance of the distinctive nature, or essential difference of cancer, a perfect definition be yet unattainable, it were much to be wished, that we possessed such a competent description of the disease, as might enable practitioners readily to discriminate its various forms, and to distinguish it from affections to which it bears the greatest affinity. For it must be obvious that, till some practical distinction be obtained, or, in other words, till precise ideas be annexed to the term cancer, practitioners in particular, who have not had much experience in this disorder, may readily confound it with other maladies, which, though apparently similar, are yet essentially different, and thus be led to the adoption of a practice which they would not otherwise have pursued.

From the want of a criterion, or some unequivocal diagnostic sign of cancer, it must likewise be ascribed, that so many impositions are daily practised on the ignorant and credulous public, by nostrum-mongers and others, from motives of cupidity and sordid views of self interest, and that various cases have been related, in which cures are confidently said to have been performed, though every person conversant with the nature and character of cancer knows how unavailing have proved all the methods hitherto proposed for the cure of this intractable disorder.

Hence

Hence likewise, we must necessarily receive, with a great degree of scepticism, the favourable reports of any new remedy suggested for the cure, or even alleviation of so hopeless a malady, however respectable the authority on which it is recommended. Nor can such scepticism, we think, appear unreasonable to any one, who reflects that, though innumerable remedies have been celebrated, at different periods, for their virtues in cancer, yet, at this moment, we know no more of its cure than was known in the days of Hippocrates and Galen.

Notwithstanding these impressions, we entered on the perusal of Mr. Carmichael's Treatise, in which the carbonate of iron is proposed as a remedy in cancer, with a considerable degree of interest, because, though attempts to cure it have hitherto proved unsuccessful, we did not consider it impossible, that others might be more fortunate, or more sagacious, than their predecessors.

Mr. Carmichael, it appears, was first induced to employ this substance in carcinomatous affections, in consequence of adopting the hypothesis, that cancer, like hydatids, is a parasitical animal endowed with independent vitality.

This belief in the animalcular nature of cancer obviously led him to the consideration, that as iron was very effectual in destroying intestinal worms, so it would prove equally destructive to other animals of a parasitical nature. The arguments, which he adduces in support of the above opinion, are chiefly founded on a loose analogy, or fancied resemblance between the structure of cancer and that of the polypus and other zoophytes; on the gratuitous assumption, that cancerous affections principally occur in parts which possess the lowest degree of vitality, and that such parts are particularly favourable to the lodgment and growth of parasitical animals.

Though the hypothesis which ascribes to carcinoma an hydatid or animalcular existence, appear, in our estimation, not less fanciful and extravagant than the arguments adduced in its support are vague and inconclusive, yet candour and philosophy suggest the propriety of suspending our judgment, as to the practice he recommends, till it has been submitted to a fair and liberal trial. In the mean time, we may be permitted to express our doubts respecting the true nature of the cases, in which this remedy proved successful. Exclusive of other circumstances, the quick relief experienced, and the rapidity with which the disease yielded to this mode of treatment, so contrary to what usually happens

happens in genuine cancer, cannot fail to excite such a suspicion in the mind of any one in the least acquainted with the history and progress of this formidable malady. This suspicion is still farther strengthened by some recent reports which, if well founded, must lead us to fear, that its fate will be similar to that of the solanum, cicuta, &c, which, it is well known, though once equally extolled, no longer hold the same place in the opinions of medical men as formerly.

But whatever may be our doubts respecting the specific powers of iron in genuine cancer, we think it cannot be denied, from the cases adduced, that it has in many instances contributed, particularly when assisted by the exhibition of appropriate internal medicines, to the cure of inveterate and phagedenic ulcers, even though these had existed for a considerable length of time, and bafled various remedies.

We lately took occasion to remark, in the Medical and Physical Journal for November, 1808, page 401, that an ointment, formed of the rust of iron with citric acid and hogslard, had long ago been employed in the French West India Islands, and was considered as a very effectual remedy in cases of yaws, even when deemed otherwise incurable.

As a farther proof of its utility, it may not be here improper to relate the following instance of its beneficial effects, in what was considered, by several practitioners, as a carcinomatous affection of the tongue.

Mr. Clunes, a taylor in Stanhope Street, about a year and a half ago, consulted me respecting an excrescence on his tongue, which he informed me, notwithstanding the use of various remedies, still continued to become worse, and excited in his mind very uneasy apprehensions.

On examination, I perceived a fungous excrescence of a foul and sloughy appearance, occupying the middle and posterior part of the tongue. It was about an inch in breadth; its edges were thick, elevated, and irregular; and near its centre was a small opening, which, on introducing a probe, was found to penetrate to some depth into the substance of that organ. It was of a pale whitish colour, and rose about one-fourth of an inch above the surface of the tongue.

Though the complaint, by his own account, was of some months standing, he could not distinctly say when it commenced, but conceived it must have existed several weeks previous to his paying much attention to it, since he perfectly recollected, that during that space of time, and before

fore he thought of consulting any practitioner, he had experienced a sense of uneasiness in the part, but had been so much occupied in executing orders, in the line of his business, as to call off his attention. This account seems extremely probable, since we know, from recent experiments, that the tongue is not endowed with that exquisite sensibility which it was formerly supposed to possess, and which we might be led a priori to conceive.

His deglutition was little, if at all, impeded by it; neither were any of the sublingual or maxillary glands apparently enlarged, though he alleged that one of the latter had been previously affected. It was not attended with much pain on pressure, but bled occasionally, when any rough or hard substance came into contact with it. The hæmorrhage, however, did not appear to have been either frequent or at any time profuse. The health and strength of the patient were evidently much impaired, owing probably to dyspeptic complaints, with which he had been for some time harrassed, as well as to the anxious state of his mind respecting the issue of the malady.

Mr. C. it must be observed, is of a scrophulous diathesis, and has, at various periods of his life, been subject to glandular swellings or obstructions in different parts of his body.

Under these circumstances he was put on a course of cicuta with calomel, from the latter of which remedies, in particular, when properly administered, and persisted in for a sufficient length of time, we had frequently witnessed the most beneficial effects, not only in ill-conditioned and inveterate ulcers, but also in glandular tumours or obstructions, whether of a scrophulous kind, or apparently approximating to a schirrous or carcinomatous nature. While country air and exercise were strictly enjoined, particular attention was also paid to obviate the deranged action of the digestive organs, and to procure a regular condition of the alvine discharge.

This plan, which was steadily pursued for some weeks, appeared at first to be productive of manifest advantage, for the health of the patient gradually improved under it, and the sore began to assume a more favourable aspect, while at the same time the excrescence indicated a tendency to separate and slough off. This, however, was but of short continuance, for the ulcer quickly again degenerated, and even assumed a more sordid appearance than before, spreading, and penetrating deeper into the substance of the tongue; the fungus also, which had acquired a dull leaden colour, became thicker and more elevated at the edges

edges, and apparently less disposed to separate. In this state of things I was induced, by Mr. Carmichael's favourable reports of his success from the internal and external use of iron in some cases of supposed cancer, to make trial of this remedy. Accordingly, while the tinctura ferri muriati, and various other chalybeates, were prescribed internally, the carbonate of iron was employed externally under the following form.

℞. Pulv. tragacanth. comp. ℥ij. Carbonat. ferri ℥ß. Aq. rosar. q. s. ut fiat massa, in trochiscos, mediae magnitudinis, formanda. Quorum unus vel duo detineantur subinde superlinguam, donec liquescant.

Stimulant and detergent gargles were likewise occasionally used, with the view of exciting the action of the affected organ, and thereby of promoting the separation of the sloughs.

In a few days after adopting this plan, the patient being under the necessity of taking a journey to Liverpool, was ordered either to supply himself with a sufficient stock of his medicines, or to get them renewed occasionally during his absence. At this time the excrescence appeared as if beginning to separate from the sound parts beneath, and hopes were entertained that it would be soon entirely thrown off; the ulcer also, though somewhat deeper and more extensive, displayed a less unfavourable aspect.

In about three weeks he returned to town, when he informed me, with joy depicted on his countenance, that his health was greatly amended, and that his tongue was nearly well. From his account it appeared, that the excrescence had separated in successive layers or sloughs. On viewing the tongue, at this period, the surface, from which these had been cast off, as well as that of the ulcer, was covered with an opaque whitish crust. He was now ordered to discontinue the gargle, and confine himself to the use of the lozenges. With this injunction he cheerfully acquiesced, because, according to him, they alone had effected the favourable change. Within a few days afterwards, the crust dropped off, and left the parts beneath perfectly healed, and the cavity in the middle completely filled up.

Since the removal of his complaint, this patient has enjoyed better health than he recollects to have done for several years before.

Whether the present affection was of a carcinomatous or scrophulous nature, different sentiments may perhaps be entertained. The last of these two opinions is certainly countenanced by the marked predisposition of the patient to such affections, who, as already observed, had often

laboured

laboured under glandular swellings or obstructions in different parts of the body. This conclusion, respecting the nature of the disease, is perhaps still farther supported by the success of the mode of treatment pursued, which was in many respects similar to that commonly considered as most efficient in scrophula.

Some practitioners have, we are aware, maintained, that an affinity subsists between scrophula and carcinoma: but we cannot see any grounds for acquiescing in this opinion; since, whatever may be alleged to the contrary, no evidence has yet been produced to substantiate the assertion. Even, however, in this view of the subject, the publication of the present case cannot, we think, be deemed wholly unimportant, since, in as far as a conclusion may be drawn from a solitary instance, it affords additional evidence of the beneficial effects of iron in promoting the cicatrization of inveterate and ill-conditioned ulcers, and leads us to hope, that it may prove a valuable auxiliary in a class of diseases both dangerous and intractable.

On the whole, though we are inclined, as already mentioned, to doubt the specific powers of iron in genuine cancer, and are of opinion, that most of the cases brought forward by Mr. Carmichael are merely those of phagedenic ulceration, yet, as he also details one or two others, apparently of a carcinomatous nature, in which a cure was effected by its use, and as, moreover, it appears to have afforded relief in those cases wherein it did not ultimately succeed, these circumstances seem to us to render it worthy of the attention of practitioners. Hence we deprecate all hasty decision on the subject, and trust no one will rashly infer, because this remedy cannot accomplish every thing affirmed respecting it, that it is of no value; for were it only productive of relief in such a deplorable malady as cancer, that alone would be sufficient to entitle it to our regard, since, certainly it is no small advantage to be in possession of an agent capable of alleviating the severe pains of a disease, though we should still have to lament the want of one adequate to its cure.

In short, whatever may be the merits of the remedy under consideration, these, it is obvious, can only be ascertained by experiments made by different individuals accustomed to investigation, and unfettered by hypotheses or preconceptions of any kind.

Δόξα μὲν ἀνθρώποισι κακὸν μέγα, πεῖρα δ᾽ ἄριστον.

London, March 1810.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

A Married Lady, about 30 years of age, of a plethoric habit, was affected in January 1803, with the usual symptoms of ascites; which, in the opinion of the two medical men who attended her, originated from a hard and long protracted labour, as it was perceived to augment gradually upon her from the above period; during the time the lymph was accumulating she again conceived, and went her regular time, and was delivered of a full-grown live fœtus. About two months after the birth of this child, and after the various medicaments had been administered, according to the discretion of the medical attendants, without the least perceptible benefit being derived, the tension of the abdomen becoming almost insupportable, paracentesis was performed; by which means, sixteen pints of a thick glutinous fluid were evacuated. What medicines were given immediately after, I was not able to learn; but whatever was administered, had not the least effect in exciting the action of the absorbents, or in preventing the effusion of lymph. She once more became pregnant, and after being in that state for three months, she was attacked with a most violent and excruciating pain of the body, extending across the left hypochondrium; she had a slight nausea at the time, with little or no thirst, and the pain extending upwards to the scapula. The medical gentlemen who first attended her were divided in opinions; one being certain that a disease of the liver existed, independant of the ascites; and the other as certainly asserted it to be a mis-placement of the same viscus. At this time, which was in March 1804, about fourteen months from the commencement of the affection, I was requested to visit her; the pulse was very languid, the system much emaciated, and she was in a forward state of pregnancy. After the cessation of the pain which affected the left hypochondrium, a violent soreness of the abdomen was the consequence, and, from this time, I dated the origin of an indurated tumour of some of the viscera; upon which I imagined the continuance of the malady mostly depended. The pain being situated immediately in the left hypochondrium, gave me a supposition it had no connection with the liver, but was a morbid enlargement of the spleen.

In

In consequence of the abdomen filling at the expence of every other secretion, and as the disease had been of long continuance, the system was so extremely debilitated, that I had but very little probability of success; and indeed, I considered it as an hopeless case, (but Providence deemed it otherwise). Notwithstanding this, I suggested a mode of procedure; the other gentlemen coinciding with me, as to its being appropriate to the case. As there was no time to lose, it was instantly executed, with an intention of removing the visceral obstructions.

Hab. Sub. mur. hydrarg. gr. ß. Pilul. sapon. gr. v. M. ft. bol. nocte maneque sumend. Hab. tinct. digital. purpur. gtt. xx. Super. tart. potass gr. xx. Decoct. cinchona, q. s. ft. haust. 8va. quaque hora sumend. Stimulating embrocations were applied to the abdomen, with friction, to augment the action of the decreased vessels, with an occasional intervention of the following:

℞. Sub. mur. hydrarg gr. v. Pulv. rad. convolv. jalap ℈j. M. ft. pulv. pro re nata sumend.

By this mode of treatment, the enlargement of the spleen gradually diminished, and became less painful; the visceral obstructions were soon removed; the digitalis excited the action of the kidnies beyond my expectation; and a great discharge of urine followed; in the course of two months, the fluid was totally evacuated, (which is rather singular, after the operation had been performed, as then it generally is more obstinate to remove); a nutritious regimen to support the vis vitæ was ordered. By this mode of procedure, and the exhibition of some tonic medicines, that valuable gift (health) was gradually restored, and she has since been the mother of two children.

I am, &c.

EDWARD LLOYD KNOWLES.

Newmarket, June 22, 1810.

To the Editors of the Medical and Physical Journal.

Gentlemen,

Medical teachers very properly urge upon their audiences, the utility of reviewing every case of importance which has been committed to their care, after it has arrived at its termination; which scrutiny, if fairly conducted, must always

be beneficial to the individual who makes it, and ultimately to the public. Such a member of society as the late Mr. Windham cannot be removed to the "house appointed for all living," without exciting attention to the circumstances which led to the solemn change particularly when those circumstances do not rank with those of every day's occurrence.

Newspaper details cannot from their very nature be upon all occasions correct, but I take it for granted that when a number of them concur in certain leading points, it will be allowed that the truth is promulgated. What they have given the world respecting the last illness and death of the great politician we have just lost, appears to me of much importance, especially to that part of the rising generation who are qualifying for medical practitioners; if you, Gentlemen, view the subject in the same light, you will oblige me by inserting these remarks in your next Journal. I feel the less reluctance in obtruding myself upon the medical public on this occasion, from the conviction that the faculty generally acknowledge the imperious necessity of medical reform; whenever this great work is *properly* attempted, it will be found indispensibly necessary to direct a portion of attention "to the artificial division of the healing art into the medical and surgical departments;" for, as it is well observed by that eminent surgeon and acute writer, Mr. Abernethy, "An evil seems to me to have arisen from this division; it has caused the attention of the physician and surgeon to be too exclusively directed to those diseases which custom has arbitrarily allotted to their care."

Mr. Windham's case (if truly given) highly illustrates the inestimable advantages of general medical knowledge when brought to the test with talents, however great, that are bounded by the too long prevailing absurd division of the healing art. We are told that this gentleman was in his 61st or 63d year; that he received a blow on the hip whilst in a state of violent bodily exertion and mental anxiety, and, as the event proved, in a state of highly unfavorable predisposition, confirmed by the observation in the report, "his powers were consumed." From all these circumstances the adoption or avoidance of the knife became a very critical point indeed for professional men to decide, who were not most intimately acquainted with the exact state of the physical condition of the subject demanding such decision. Well and truly might they say, "that the knife was at all times to be used with awe, and never but where life was absolutely at stake, or where the probable prolongation

of

of life was to be made endurable by relief from pain." Though separately consulted, a majority concurred "that there was no danger in avoiding the operation, but much in undergoing it," for the tumour was no serious inconvenience, and scarcely accompanied with pain." But other *surgeons* gave him an opinion that the tumour might be safely cut out, and he instantly resolved on the experiment; resolved on making his life the test of experiment: nor would he wait for any preparation of the system; a refusal he would never have made, had a degree of medical sagacity existed in the minds of some of the practitioners consulted, sufficient to point out the indispensable necessity of such a preliminary step to this and nearly every other surgical operation; but as Lord Milton observes in his eulogy, "nothing could swerve or bias him in the opinion he once had formed". How much those who esteemed him, have to lament that such firmness (as it is termed) did not receive the most rational bias at first, there is too much cause to apprehend from the whole narrative, that his active mind was not clearly directed to the primary consideration, namely, his general health, previous to and at the time of receiving the blow; also from that event to the period of consultation. Had he been sufficiently impressed with the solemn, irrefutable truth, that such a view and strict inquiry was to determine the just treatment of his case, and that the accidental tumour and its local management were mere secondary considerations, it is probable the ultimate event might have been somewhat different; for what will not an ardent mind rightly directed perform? unless it be fact, as stated, "his powers were consumed;" then indeed it is needless to add a word on the consequences of a severe manual operation, although performed with the "most perfect skill," as doubtless this was.

But it may be unadvisedly said, the tumour was the cause of all the physical disturbance, and nothing short of its immediate removal could afford the sufferer a hope of recovery; but is or can such a case be made out? Has the event, upon the enjoined review of the case now it is come to pass, justified the treatment? When are we required by imperious unavoidable necessity, to remove tumours "that give no serious inconvenience, and are scarcely accompanied with pain?" From various external causes, parts have doubtless become extraneous, and from inattention in the sufferer, or defective *medical* ability in the surgeon, they finally require removal, or nature will be as it were teased to death in her unavailing efforts to free herself from the

incumbrance

incumbrance; but then the accompanying general symptoms are sufficiently unambiguous to all who devote attention to them, and by no means such as we are told (if correctly stated) existed in Mr. Windham. In his case the blow could only be the simple exciting cause, acting upon unfavourable physical condition, to produce one of those "non-descript diseases or varieties of local disease," absolutely in its origin, symptoms, progress, and termination, dependent upon the constitution and general disturbance of the health, which can produce "so many varieties, and even every variety," and upon which, medical sagacity, judiciously exercised, will on almost every occasion, be as competent as it is necessary to decide.

Now, when the result of that decision is perempt, "that there is no danger in avoiding a manual operation, but much in undergoing it," is the honour of a noble science, like that of medicine, to be committed by the obstinacy of any energetic, resolute man, be his rank what it may? There is no doubt but thousands of medical practitioners daily resist the dangerous importunities of ignorance or ill directed courage, much to the advantage of their patients and the glory of our art, never losing sight of the wholesome precept, "stand in due awe of the knife; it is not to be used but when life is absolutely at stake." *Their* language to Mr. W. would doubtless have been, the blow being received during an unfavourable state of the system, caused a greater or less degree of comminution and destruction of vessels, with a rude shock to the subjacent bone; rest, and the efforts of nature alone, in a firm healthy physical condition, have been equal to the counteraction of serious consequences from far more extensive local mischief than this; but we find such not to have happened in your case, consequently you are not in that healthy, physical condition you should be; still do not suffer a knife to approach this swelling, but attend to your general health; mark the result of the vigilant use of medicine and general means; if you do any thing externally, by way of aid to these means, apply mild soothing fomentations, absolute rest, gentle frictions promotive of absorption, leeching, &c. Should these means fail, and your "powers are consumed," they will at least be a test of the fact, and prove that the knife may hasten the termination, but cannot possibly prolong your existence; if from other advisers, aided by your own daring impatience, you are determined on an operation, and its consequences, we remonstrate, we combine against such a rash, unqualified, unjustifiable determination,

determination, because you are not in a physical state to undergo it, because the mere surgical operation is the dernier, not the primary resource of our art, never to be adopted but from necessity and in doubtful or ambiguous cases with awful reluctance; but when decidedly indispensible with calm courage and well grounded hope: in your instance no such necessity exists, therefore we protest against any such a dreadful perversion of the views and resources of so incomparably valuable a branch of science as that of medicine.

I am, &c.

GEORGE NESSE HILL.

Chester, July 13, 1810.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

ANOTHER very accurate and ready test for arsenic has just occurred to me, which, being easily performed, requiring neither expence, apparatus, nor a nice acquaintance with operative chemistry, may deserve the attention of your readers and the profession at large. It is, however, more suitable to detect the *dry* oxide itself than when in solution, and, therefore, as this poison is, I believe, always administered in powder when some fatal purpose is to be effected, the method I shall now suggest will consequently be found more practicable though not more certain than the former; but, at all events, one will serve to corroborate the other, for, when we can avail ourselves of an extended evidence in criminal conviction, nothing should be lost, nothing neglected.

From the difficult solubility of the white oxide, especially in cold fluids, we may in most cases obtain, from one source or other, a sufficient quantity for examination, for it is astonishing how very delicate these tests are, particularly the first, which will render the effect obvious when water is even slightly impregnated with the poison, so that a mere fractional part of a grain could not escape notice.

As an instance of this second process, let us take about a grain of the white powder, or suspected arsenic; mix this very intimately with two or three grains of nitrate of potash;

potash; introduce this mixture, by the point of a pen-knife, into a small phial, in such a way, by inclining the phial horizontally, that the powder may remain undisturbed on the side or thinner part of the glass: now, apply the flame of a small lamp just under the powder, and presently, if there be arsenic, the nitrate will be decomposed, nitrous gas and nitrous acid evolved, and arseniate of potash will remain as the result.

I would prefer spirit of wine to oil, for the lamp, as it produces no smoke that can tinge the phial and intercept our seeing distinctly the yellow fumes peculiar to nitrous gas when in contact with the atmosphere. The acid vapour is easily recognised, either by its smell, by presenting the point of a feather imbued with solution of ammonia, or even by placing a morsel of moistened litmus paper within the phial and over the materials during this short operation.

As a farther support of this test, proceed thus with the new product, the arseniate of potash—fill the phial with warm rain water, that some of the salt may be dissolved; then, to the mere surface of the solution, present a piece of dry nitrate of silver (lunar caustic), and the peculiar dark brick-coloured precipitate, which I described in my last communication, will fully evince the fact, that arsenic had been administered.

Since either of the schemes I have proposed must soon become familiar and practicable, I trust your Correspondent, Mr. Jones, will, at his leisure, again submit some of the powder to trial; and I have no doubt he will readily favour us with a faithful report, whatever the result may be. I cannot quit my pen without acknowledging his kindness in offering the remainder of the powder for analysis; this I would willingly undertake, did I not implicitly depend upon his own accuracy, when assisted by the new means I have announced, as well as on his candour and liberality in confessing that the poison actually does exist in the same powder, should he succeed in the research.

I am, &c.

JOS. HUME.

Long Acre, July 13, 1810.

Corrigenda. In the second paragraph of my last letter, after *precautions*, add, *were taken.*

To

To the Editors of the Medical and Physical Journal.

Gentlemen,

In January, 1808, Mrs. B——— having some obstruction in the urethra, attempted to relieve it by the introduction of an iron bodkin, which, slipping from her fingers, escaped into the bladder.

Soon after this accident, symptoms of inflammation of the bladder came on; a constant desire to make water, attended with almost insupportable pain, arising from the contraction of its muscular coats on the sharp ends of the bodkin.

Several weeks had elapsed from the introduction of the instrument to the bladder, before Mrs. B. became my patient, whose sufferings had much reduced her health, and had induced high symptomatic fever.

I commenced my process of cure by injecting into the bladder a pint of thin mucilage, with an ounce of olive oil, which was ordered to be retained as long as possible; and a bougie as large as the urethra would admit, was also retained till the discharge of the fluid from the bladder rendered it necessary to be withdrawn; when that took place, another injection was thrown in, and the urethra distended by a bougie as before. This plan was continued for a fortnight, care being taken to throw in an injection as soon as the last had come off, and to increase the size of the bougie every day, in order to produce mechanical distention of the urethra; at the end of this time, the irritation had ceased, and the urethra was sufficiently distended to allow the introduction of a pair of forceps, with which I extracted the bodkin. It was of iron, about two inches long; and it is worthy of remark, that it was thickly encrusted with stony matter, except about a quarter of an inch on each point, which was free from the calculous covering; this must have arisen from the bladder having contracted so firmly on the points as to have prevented the deposit from taking place.

The bodkin has been seen by Dr. Parry and several other professional gentlemen who were unacquainted with the case.

I am, &c.

JOHN BUSH.

Tiverton, Bath, July 16, 1810.

To

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

IN the last Number of your Journal, I see that you have inserted a Report from the National Vaccine Establishment, which differs so widely from my observations on that subject, that I have been induced to hand you the following statement, of which you are at liberty to make what use you please.

In the Finsbury Dispensary, St. John's Street, Clerkenwell, it has been my practice for the last four years, to vaccinate the patients, or to inoculate them for the small-pox, agreeably to their own choice; nor have any means been taken to bias their judgment; within the last twelve months, only four patients have been vaccinated, and upwards of one thousand have been inoculated for the small-pox.

I am, &c.

JOHN TAUNTON.

Greville Street, Hatton Garden,
July 4, 1810.

CRITICAL ANALYSIS
OF THE
RECENT PUBLICATIONS
IN THE
DIFFERENT BRANCHES OF PHYSIC, SURGERY, AND MEDICAL PHILOSOPHY.

An Essay on the Nature of Scrofula, with Evidence of its Origin from Disorder of the Digestive Organs; illustrated by a Number of Cases, successfully treated, and interspersed with Observations on the general Treatment of Children. By RICHARD CARMICHAEL, Surgeon. 8vo. pp. 111.

IT has become of late so fashionable to attribute various diseases of the human body to a derangement of the functions of the *chylopoetic viscera,* that it excited no surprise in us to find an attempt made to refer scrofula to the same source; indeed, the functions of the digestive organs so readily become disordered, and their derangement so frequently *accompanies* most constitutional diseases, that

that with our present imperfect knowledge of cause and effect, nothing is more easy than boldly to charge them as parents of the many untoward symptoms that denote any particular disease, while they only participate in common with other parts of the frame in numerous evils arising from some general cause. The greater the obscurity in which the nature and causes of any particular disease are involved, the more boldly may the assertion be made of that disease; scrofula therefore opens a wide field for speculation, and our author has availed himself of it with no little dexterity: it becomes us, however, to examine upon what grounds his hypothesis rests, and to bestow an attention to Mr. Carmichael's treatise, proportioned to the reputation he has already acquired, for we ought not to suffer doctrines to be promulgated by authority merely, which are not supported by facts and established by reasoning fairly deduced therefrom.

One of the most obvious requisites to a fair discussion of any subject is a clear comprehension of the terms employed; it becomes then an important question, what is the precise disease meant to be included under the name of scrofula? Here we find ourselves much at a loss for want of a definition by the author, for he shifts his ground so frequently, and takes so much for granted, which should have been proved, that we know not how to meet him. Assuming those symptoms to be scrofula which have never yet been allowed to be so, and which we should deny to be any part of that disease, and inferring, that because these disputed symptoms appear together with certain other symptoms, they are caused by these latter, and that, as they all disappear under a certain treatment, *scrofula* is cured by that treatment, is a mode of reasoning neither fair nor correct, yet, upon this, and no better foundation, rest some of our author's peculiar doctrines in the treatise before us.

Among those who have chiefly contributed to hand down and propagate errors respecting this disease, the author would place in the foremost point of view Dr. Cullen.

"In support of this statement, it is only necessary to recollect his opinion that scrofula arises from "a peculiar acrimony of the fluids," that the disease 'rarely appears but in children whose parents had at some period of their lives been affected with it;' and his belief, that when it fails to appear in the children of scrofulous parents, it may discover itself afterwards in their offspring in the succeeding generations; as *also his reliance* upon mineral waters, and his supposition that they produce their beneficial effects by "washing out the lymphatic system."

We transcribe this passage to notice a mistake Mr. C. has fallen into, as to Dr. Cullen's *reliance* upon mineral waters, for the cure of scrofula, and we shall let the Doctor speak for himself on this subject. "For the cure of scrofula, we have not yet learned any practice that is certainly or even generally successful. The remedy which seems to be the most successful, and which our practitioners especially trust to, and employ, is the use of mineral waters; and

and indeed, the washing out, by means of these, the lymphatic system, would seem to be a measure promising success; but in *very many* instances of the use of these waters, *I have not been well satisfied* that they had shortened the duration of the disease more than had often happened, when no such remedy had been employed."* Dr. Cullen states scrofula to be an hereditary disease, and Mr. Carmichael strongly objects to this opinion; but before we go further, let us see if Dr. Cullen and Mr. Carmichael really mean one and the same disease. We shall give the author's own description of the disease he has been so successful in treating, and his method of cure.

"In entering into a consideration of the circumstances which in children may produce disorders in the alimentary canal, it may be satisfactory to state a few cases, which prove that glandular swellings of the neck in infants are preceded and accompanied by a disordered state of the bowels; and that the removal of the former depends upon relieving the latter. Infants on the breast and in the second year are frequently affected with swellings of the glands at the upper and lower parts of the neck, and in the axilla, which make their appearance suddenly, and increase rapidly, often to a considerable size, until suppuration is established: when they break, or are punctured, they are difficult to heal, remain a long time open, and though at first they evince more of the phlegmonous than of the scrofulous inflammation, yet afterwards they often assume the *characteristic* appearances of scrofula. Having observed the connection between disorder of the primæ viæ and the symptoms of scrofula, I was induced to inquire into the state of the bowels in these cases; and I universally found that the evacuations of the child were either green, black, or slimy, for some time previous to the occurrence of the swelling. Within the last seven months I took notes of several of these cases which occurred at St. George's Dispensary, and in private practice; and I shall transcribe a few of them, as their history must convey a clearer idea of their nature than any general description.

"Case 1.—In the beginning of July, 1809, I was called upon to see an infant, three months old, affected with large tumours of a phlegmonous appearance on each side of the neck, immediately below the under jaw. On inquiry, I was informed that the child from her birth had never been regular in her bowels, and that her evacuations were constantly either of a green or black appearance. I directed a grain of calomel with six of rhubarb to be given every second night, and ten grains of prepared carbonate of lime, with two of carbonate of soda, every morning and evening.

"This course had soon a good effect. In four or five days the evacuations became natural in appearance, the tumours were poulticed

* First Lines, § 1753.

ticed until they broke, and in about a fortnight were perfectly healed.

"Case 2.—Edmond Burke, 14 months old, was brought to St. George's Dispensary on the 21st of August, 1809, on account of a tumour, hard, red, and painful, extending under the lower jaw from one ear to the other, the commencement of which was only observed by the mother four days before. His dejections, during the preceding six weeks, were of a green and sometimes of a black colour. I directed emmollient poultices to the tumour, a grain of calomel, with ten of rhubarb, to be taken every second night, and fifteen grains of carbonate of lime with five of soda, o rning and evening.

"Under this treatment, the evacuations soon exhibited a natural appearance, the tumour broke in a week, and after discharging a considerable quantity of matter, healed in a fortnight more, like any common abscess.

"I pointed out this case, as well as several others of the same description, to the attending physicians of St. George's Dispensary.

"Case 3.—William Jackson, 5 months old, was brought to me on the 21st of September, 1809, with an inflammatory tumour on the left side of the neck, which was only observed seven days before. His mother informed me that his bowels had not been regular since his birth, but that he was attacked frequently by convulsions during the last fortnight, and that his dejections were of a green colour and sour smell. He was directed to take half a grain of calomel, with ten grains of prepared carbonate of lime, morning and evening. The frequent use of the tepid salt-water bath was also enjoined, together with warm cloathing and frequent hand rubbing.

"Under this treatment, in a fortnight, the evacuations became perfectly natural; about the same time the tumour broke, and soon afterwards healed without difficulty.

"Case 4.—I was consulted about a child, 4 months old, on the 5th of October, 1809, who was affected with a large swelling, similar to those already mentioned, on the left side of the neck. She had been ordered poultices of sea-weed, by a physician of this city, I presume from an opinion of the scrofulous nature of the tumour.

"The mother requested advice at the same time for a disordered state of the infant's bowels, which she said had never been regular since her birth: under the plan of treatment mentioned in the last case, both complaints were removed in four weeks."

It certainly is no uncommon thing to meet with such phlegmonous tumours as the author describes, in young children, *accompanied with* a disordered state of the primæ viæ; we have seen numerous cases of them, and have for many years treated them nearly in the same manner Mr. Carmichael recommends, but we never

never flattered ourselves that we had *cured scrofula* when these tumours suppurated and had healed; first, because we believed scrofula to be a disease that did not usually appear in the *first months* of the child's life; and next, because real scrofula is too unmanageable a disease to be cured in *two or three weeks*; scrofulous tumours are also much more indolent than those above described, which make their appearance suddenly, and increase rapidly, and as the author justly observes, "evince more of the phlegmonous than of the scrofulous inflammation;" they also appear at all seasons of the year, and in children apparently of very different temperaments, circumstances differing materially from the characteristic appearances of scrofula; but not to rest on our own authority, let us compare this affection with the disease named scrofula by Dr. Cullen, for to the accuracy of his history and description, we presume Mr. Carmichael will not object, whatever he may think of his opinions on the nature and causes of that disease.

"The scrofula generally appears at a particular period of life. It seldom appears in the *first* or *even in the second* year of a child's life; and most commonly it occurs from the second, or as some allege, and perhaps *more properly*, from the third to the seventh year. The scrofula generally shows itself at a particular season of the year; and at some time between the winter and summer solstice, but commonly long before the latter period. It is to be observed further, that the course of the disease is usually connected with the course of the seasons. Whilst the tumors and ulcerations peculiar to this disease, appear first in the spring, the ulcers are frequently healed up in the course of the succeeding summer, and do not break out again till the ensuing spring, to *follow again with the season, the same course as before.*" The tumors are without pain, and without any change in the colour of the skin; in this state, they often continue for a long time; *even for a year or two*, and sometimes longer: from the time they first appeared in the spring, they often continue in this way till the return of the same season in the next, or, perhaps, the second year after. About that time, however, or perhaps in the course of the season in which they first appear, the tumor becomes larger and more fixed; the skin upon it acquires a *purple*, seldom a clear redness, but growing redder by degrees, the tumour becomes softer, and allows the fluctuation of a liquid within to be perceived. All this process, however, takes place *with very little pain* attending it."*

We may fairly conclude, the *hard red painful* tumours Mr. Carmichael so easily and speedily cured, were not scrofula; this trifling circumstance, however, is no obstacle to our author, in constructing his hypothesis, which he attempts to establish by the following argument.

"But even supposing that these tumours were not in any way entitled

* First Lines, § 1740, 1742, 1744.

entitled to the name of scrofula, their connexion and dependance upon disorder of the alimentary canal, sufficiently establishes the point, *that swellings of the lymphatic glands may arise from, or be connected with, disorder of the digestive organs;* and this is all that is at present necessary for our purpose in tracing the symptoms of scrofula to their source."

We do not see how the admitting the connexion and dependance of those tumours upon disorders of the digestive organs, is *all* that is necessary to trace the symptoms of *scrofula* to their source, while we suppose these tumours *not in any way entitled to the name of scrofula.*

Resting then upon the solidity of the above quoted argument, the author proceeds, "Having satisfied ourselves that the first symptoms of scrofula are those which denote disorder of the chylopoietic viscera, and that disorder of those parts is in early life frequently followed by swelling of the lymphatic glands of the neck, let us now proceed to consider the several circumstances, which in childhood precede and induce this effect."

We must here observe, that *we* are not *satisfied* of the author's position; and lest our readers should be as little satisfied with it as ourselves, we will, by an extract from the volume before us, place the author's doctrine in a more connected point of view, and offer a few remarks, which naturally arise from the consideration of the proximate cause of scrofula being a disordered state of the chylopoietic organs.

"When it is considered that the first symptoms of scrofula are an enlarged belly, swelled upper lip, irritation at the nostrils, and irregularity of bowels, with green and black coloured fæces, all of which continue during the progress of the disease, and severally denote disorder of the digestive organs; and when it is also considered that disorder of those organs in children, is accompanied by the generation of an acid in the alimentary canal, we are justified in considering scrofula as a disease arising from, and generated by, disorder in the bowels, and that the treatment most likely to be successful, would be grounded on the indications of neutralizing the acid formed in the primæ viæ by alkalies and absorbent earths, of promoting a healthy secretion of the gastric juice, and of the bile, by exercise, tonics, and mercury, and of preventing, as much as possible, the further formation of acid in the bowels, by restricting the patient to a diet easy of digestion, and free from ascescency or disposition to the acetous fermentation."

So far from considering the symptoms above enumerated, as the pathognomonic ones of *scrofula*, we should conclude them to denote the presence of worms in the intestinal canal, or at least such a state of that canal as readily affords a nidus for those insects; while the healthy state of the digestive organs, during the existence of indurations of the glands and disease of the bones, the *acknowledged* symptoms of scrofula, would induce us not to think ourselves "justified in considering scrofula as a disease arising from, and ge-

nerated by, disorder in the bowels." At all events, the scrofula of Mr. Carmichael differs from the scrofula of other authors, especially from the disease so called by Dr. Cullen, as we have endeavoured to show; but there yet remains one point on which we must compare our author's opinion of scrofula, and Dr. Cullen's, and that is, its being, or not, an hereditary disease. The author thinks the popular notion of scrofula being hereditary, is founded in error, and he charges Dr. Cullen with propagating this error; he says, "in support of this statement, it is only necessary to recollect his opinion, that scrofula arises from a peculiar acrimony of the fluids;" that the disease rarely appears but in children whose parents had at some period of their lives been affected with it. In this quotation, *opinion* is confounded with fact; scrofula arising from a peculiar acrimony of the fluids was Dr. Cullen's *opinion*, its being hereditary was asserted as a matter of fact, and is only to be controverted by disproving that fact, by showing scrofula does not occur more *generally* in the children of scrofulous parents. Dr. Cullen's words are plain and unambiguous, and are capable of confirmation or refutation by an appeal to experience. "With respect to the influence of parents in producing this disease, it deserves to be remarked, that in a family of many children, when one of the parents has been affected with scrofula, and the other not; as it is usual for some of the children to be in constitution pretty exactly like the one parent, and others of them like the other, it commonly happens, that those children who most resemble the scrofulous parent, become affected with scrofula, while those resembling the other parent, entirely escape."* After all, it is a mere dispute of words; the author, while he admits advanced scrofula *to prevail* in particular families, denies the disordered state of the digestive organs, and the generation of acidities in the primæ viæ in children to be hereditary, and Dr. Cullen never asserted them to be so. The author calls that state of those organs scrofula, and Dr. Cullen never did. The following attempt to explain the fact of scrofula prevailing *in particular families*, is not satisfactory, nor does it notice the disease appearing exclusively in the children resembling the scrofulous parent.

"It must be acknowledged, however, that there are instances of the prevalence of this disease in particular families; but when this is the case, it is most probable, that instead of an hereditary taint, either a languid inert fibre predisposes to its attacks, or the same bad mode of rearing children (such as abandoning them to the negligence of hireling nurses) has been transmitted from one mother to another, and produces similar effects whenever it occurs."

With respect to the exciting causes of scrofula, much needs not to be said; while the author persists in calling weak digestion and acidities in the primæ viæ scrofula, he may fairly call all circumstances

* First Lines, § 1739.

stances which contribute to the production of these exciting causes of scrofula, and they are too well known to require enumeration; one *fact* also renders the subject rather complex, scrofula being a disease of debility, and the persons in whom it is to occur in future life, being generally very weakly in infancy, digestion in them is imperfectly performed, and acidities are produced; the scrofulous symptoms which afterwards appear, are then attributed by the author to this acidity, and weak digestion as its *proximate cause;* although during the existence of these scrofulous symptoms, neither weak digestion nor acidities can be discovered. How easily scrofula may be produced, and by what a variety of causes may well be imagined; and the author says, "in one of the worst cases of scrofula I have met with, and which continued even to manhood, the disease was attributed, by the mother of the patient, to his eating *a large quantity of manna* that was accidentally left in his way."

The treatment of scrofula will of course be founded upon the opinions of the author previously delivered, and a summary of it is contained in the following quotation:

"Having so far ascertained that indigestion is the proximate cause of scrofula, and that the remote causes *are weakness of fibre,* a want of due exercise, damp cold air, and acescent diet; the indications of cure are evidently to restore the digestive organs to the due exercise of their functions by the necessary attentions to diet, air, and exercise, and by the administration of medicines capable of correcting the morbific products of indigestion, and of exciting a healthy state of the digestive secretions."

The weakness of fibre which is here described as a *remote cause* of indigestion, has generally been considered as a predisposing cause of scrofula; whatever therefore will contribute to remove this weakness of fibre, will certainly be useful in the treatment of scrofula, and we approve of the exercise and other strengthening means recommended by the author, without assenting to the accuracy of his notion respecting indigestion being the proximate cause. The cases given in the former part of the book, exhibited the mode of treatment adopted by the author in the bowel complaints of very young children, and he has in addition given us several cases *of real scrofula,* treated in the same manner; and we shall now examine the result. We call it real scrofula, because in most of them the disease occurred at the usual age, viz. from five or six years to adult age, and the symptoms were such as have *hitherto* been denominated scrofula.

"I usually commence my plan of treatment by the exhibition of a purgative, in order to meet the symptoms which denote a foul and overloaded state of the primæ viæ; for this purpose I prefer calomel, because, while it empties the bowels, it induces an increased secretion of bile, by its *peculiar* effects upon the hepatic system. Two grains of calomel may be stated at the medium dose for children under twelve years of age, combined with ten of rhu-

barb. To adults I usually give three grains, and always exhibit the medicine at bed time, as it will be more likely to remain a longer interval in the bowels while the patient continues at rest, and is not exposed to any variation of temperature. Calomel given in this manner, increases the secretion of bile before we can suppose it to have entered the circulation, and through that channel affected the liver.

"In the morning following the exhibition of the calomel, a dose of vitriolated magnesia, proportioned to the age of the patient, generally produces four or five evacuations. For the first eight or ten days, I repeat the calomel every second night, and the neutral salt the following morning, which never fails to lessen the tumid state of the belly, and the swelling and itching of the nostrils and upper lip. The patient, instead of being reduced by these evacuations, is really improved in appearance, health and spirits.

"This plan does not interfere with the exhibition of the carbonates of soda and lime, which I give twice or three times daily to children under twelve years of age, the medium dose of which may be stated at half a drachm of the former to double that quantity of the latter. To those more advanced in life, I give these medicines in as large doses as the stomach can bear without inconvenience.

"After the first week or ten days, I generally omit the neutral salt entirely, and exhibit but one grain of calomel every second night, with ten grains of rhubarb; but in this I am determined by the degree of tumefaction of the belly, and swelling of the lip, invariably persisting in the neutral salt as long as these symptoms continue. Bitters or tonics I seldom employ, because their exhibition with the other remedies would be apt *to cloy or overload the stomach*; besides, I conceive that bitters can only be of service as a substitute to the bitter resinous principle of the bile, when that secretion is vitiated or deficient. And if the mercurial stimulus is sufficient to promote a healthy secretion of bile, the use of bitters would be superfluous; and I have had sufficient experience to satisfy me, that the remedies I have recommended are in general sufficient to answer every necessary purpose."

This mode was followed by the most beneficial consequences; in three or four months, the symptoms receded, and except in a few obstinate cases, the patients are reported *cured*. It must be observed, however, that this plan of treatment was begun in all of these cases, in *June* 1809. Well knowing that the symptoms of scrofula, spontaneously recede at the latter end of summer, to return again the following spring, we require a longer time than has yet elapsed to convince us these patients have been cured. Indeed, in one of them (Ward), the ulcer of the leg had broken out in the January following; this is the case which was produced by eating manna. Several of the cases here described, occurred in the girls at a parochial school, and the author attributes the disease in them to their being deprived of their accustomed exercise. "In *a short time* from the commencement of this sedentary life, scrofula

la began to make its appearance, and afterwards affected near a third of their number." It certainly is against the generally received opinion that scrofula can be produced in so short a time, by such a cause, in habits where there does not exist hereditary predisposition; and this appears more unlikely still, if we admit indigestion and acidities in the primæ viæ to be the proximate cause, for then we should suppose causes applied *immediately* to the organs of digestion would be more speedily followed by their proper effects; yet in the boy who ate a great quantity of manna, thereby instantly disturbing the digestive organs, scrofula did not appear, the author informs us, till *the following year.*

Ever since the author has been *satisfied* that scrofula depends upon the deranged action of the chylopoietic viscera, he has not failed, in the cases that came under his care, to make inquiries concerning the state of these organs. As a proof that his *opinions* are not to be overturned by trifling circumstances, we give the following quotation.

"To my inquiry concerning the state of her bowels, I was informed that she was regular, but *this I am inclined to doubt;* for, from *the view we have taken* of the effects of exercise and pure air, in promoting the action of digestive organs, *I cannot think it probable* that digestion could have been performed with the same regularity in a young person, who was deprived of the exercise and purity of air, to which she had been accustomed from her infancy."

The impression that remains on our mind in closing the volume is, that the plan of treatment recommended by the author in infantile diseases, is proper and judicious, little differing from that generally adopted by medical practitioners; that its utility in the advanced stages of scrofula is at least ambiguous, further time being required to make a fair estimate of it; and that the hypothesis of scrofula depending solely upon the deranged functions of the chylopoietic viscera as its proximate cause, to the exclusion of hereditary predisposition, is not only unsupported by, but is directly contrary to the actual and acknowledged facts in the course of that disease.

The Edinburgh Journal, No. 23.

Art. 1.—*Medical Report for Nottingham.* By James Clarke, M. D. Physician to the General Hospital, and to the Vaccine Institution.

Dr. Clarke has here presented us with another Medical Report, which, like the former ones, is a proof both of his indefatigable industry and attentive observation. After some remarks upon the revolution which medical opinions occasionally experience, and the desiderata to be had in view by writers of future accounts of the Walcheren Fever, he has given us a case of Diabetes, which, notwithstanding a great variety of treatment was adopted, at length proved fatal. The dissection showed, what might have been expected from the course of symptoms, organic disease of the

 bladder,

bladder, schirrus of its neck, and a cartilaginous structure of the urethra. Bleeding appeared to afford at least considerable relief to this patient, and Dr. Clarke seems to regard this remedy with a favorable impression. But what we are particularly inclined to attend to in this article, is the new theory of the disease suggested by Dr. Clarke, which we shall give to our Readers in his own words.

"It has been already observed, that in most of the cases of this disease, the individuals have been previously subject to profuse sweating, which was suddenly checked by external cold, by drinking cold liquids, or by eating acid fruits, and speedily afterwards the diabetic affection appeared. Reflecting on this circumstance, it appears very rational to conclude, that the kidney became the receptacle of this suppressed fluid, and that the sweating, not simply the perspiration, was kept up from the kidney. This revulsion, as the ancients would call it, is admitted to occur from the kidney to other organs, and the converse must be equally correct. 'When the urine is not excreted, on account of some defect of the kidnies, ureters, or bladder, it has been exhaled into the skin, ventricles of the brain, or into the whole cellular fabric. The perspirable matter of Sanctorius, though so fluid, is sent off by the urinary passages, and by fear, or by medicines, through the excretory villi of the intestines.' (Haller's Physiology, 8th edit. p. 100.) The kidney will then have to throw off this sweating fluid in combination with the urine, which, from the stimulus given by this unnatural irritation, would be secreted in greater quantity; and to this stimulus the urinary organs cannot become habituated, as it is to be supposed it will be subject to the same variation as the cuticular discharge, sometimes being mild, at other times irritating; and on this principle, the constant irritation of the urinary passages, and the phymosis, may be explained. In consequence of this increased action, the circulation in the kidney will be quickened; a greater supply of blood will be required. Hence the stomach is called into inordinate action, and the demand for food proportionably increased; the appetite becomes craving; it is no sooner gratified than digestion hastily commences; chyme is formed, from which the chyle is quickly separated; but from the urgent demand of the kidney, it cannot be completely formed into blood, and in consequence, passes into the kidney with the blood, and there mixing with the urine, gives to it the saccharine quality; thus we may explain the bulimia, dyspepsia, and sweetness of urine. 'Hence the increased celerity of the blood so easily forces the red globules through these tubes, (uriniferous tubes) and, by morbid relaxation, they transmit the true fat and the chyle, and the salts of the meat and drink.' (Haller, p. 385.) Whilst this increased action is kept up in the kidney, every successive increase of debility in any other organ, will throw the more on the kidney. The stomach suffers first, being kept in constant action, and irritated by the gastric juice, which the constant

stant sense of hunger calls into this cavity; and the more action required from this organ, the quicker must be the circulation thro' it; and if the supply of blood be not adequate, and the balance preponderate in favour of the kidney, the stomach must suffer doubly by the irritation of the gastric juice, and the want of support, which at last render it unable to digest its contents, and the patient dies from inanition."

It frequently happens, that the most obvious and insurmountable objections to an hypothesis escape the notice of its framer; and we are apt to think this has been the case with Dr. Clarke: we trust, therefore, he will excuse our pointing out a circumstance or two which make against his opinions, and of which we must see an explanation before we can become converts to this new theory; for however ingeniously it is supported by its author, it yet appears liable to some difficulties and doubts, which we should thank Dr. Clarke to answer. If the increased flow of urine, in the first instance, is merely a vicarious discharge; if nothing but the "sweating fluid" is sent to the kidneys, to be thrown off by them in combination with the urine, we see no possible source of mischief, nor can we allow that any *unnatural irritation* is thereby produced, the two fluids being so exactly similar, we may almost say identically the same; if also we suppose urine to be secreted in greater quantity, we should not expect the constant irritation of the urinary passages and phymosis to be referable to this circumstance, since we should apprehend these symptoms would rather take place from a greater concentration of that fluid than from its increased quantity and greater dilution.

Admitting the increased action of the kidnies to take place, we do not see why a greater supply of blood will be required for the system, so that the stomach must be called into inordinate action, and the desire for food proportionably increased, without time being allowed for its complete formation into blood. Were this true, the same effect must follow from any excessive discharge or waste of blood from the system; in weakness produced from hæmorrhages, for instance, a greater supply of blood will be required; and if the demand for it is too urgent to allow sufficient time for the complete conversion of the chyle into blood, the chyle must be hurried through the circulation into the kidnies, for by that source most fluid matters not converted into blood are discharged, and diabetes would be produced. In extensive local inflammations, of whatever description, there is increased action, and the circulation in the part is quickened, a greater supply of blood to the part is therefore required; yet we do not find the stomach called into inordinate action, nor the demand for food increased. The stomach being irritated by the gastric juice, *which the constant sense of hunger calls into the stomach*, is certainly a novel idea, but perhaps not a very just one. Wherein resides this sense of hunger? and how it is produced? The converse of the proposition may be

 true,

true, and the constant sense of hunger be produced by the presence of the gastric juice, perhaps in a vitiated state from some disease of the secreting organs.

The treatment to be pursued in adopting this theory, is thus detailed.

"In the commencement of this disease, every thing should be done to turn the cuticular discharge into its proper course; this is to be effected by bleeding, the warm bath, and the administration of nauseating doses of tartarised antimony, ipecacuanha, and other medicines that possess the power of acting on the skin, without proving diuretic. Thus, the acetite and nitrate of potash would be bad medicines in this case. This treatment is to be assisted by a spare diet of animal and vegetable food. Having thus restored the natural secretion to the skin, nothing more remains than to moderate it, and to adopt the prophylactic measure, to prevent a recurrence of the same disease. But should this plan not succeed, and the disease baffles our means, it must be considered chronic, and now depending on a laxity or debility of the extreme vessels in the kidney; which opinion, the appearance of the kidney, and the state of the blood vessels, tend very much to confirm. It is then to be treated by astringents, tonics, blisters to the loins, the warm bath, and nourishing diet."

Next follows an interesting case of Hemicrania, which proved fatal. Upon dissection there was discovered in the posterior lobe of the cerebrum a hard tumour, which measured, after the contents had been evacuated, in length two inches, in breadth one inch and a half, and in depth one inch and a quarter; it was firmly attached to the tentorium.

Dr. Clarke speaks of the advantage of a clear diagnosis *between* tumour in the brain and hemicrania; we should rather say, in cases of hemicrania it would be very desirable to ascertain if it depends upon organic disease of the brain; for whether there is a tumour or not, the symptoms still constitute hemicrania, of which the cause may be various.

Two cases are then given, in which Dr. Clarke's treatment appears to have been very judicious, and was attended with success. They are denominated Cases of Water in the Brain; but some doubts may reasonably be entertained if water was *actually contained* within the brain, especially in the case first described, since the symptoms are equivocal, and not decidedly diagnostic of that affection. The same observation applies to the next and last case contained in this Report, A Case of Dropsy in the Pericardium.

In cases of such ambiguous nature as these, conjecture is often the utmost we can attain to, and our treatment must be founded rather upon experience of what has been found useful in similar cases, than deduced from our actual knowledge of the real nature of the disease. The paper is concluded with a few observations on the importance of accurate attention to the circumstances necessary to be observed in the gathering and preserving Digitalis, in

which

which we entirely coincide. Physicians are frequently disappointed in their expectation of the effects of this medicine, from want of its being dispensed in a sufficiently active state by those to whom this branch of the art is entrusted.

Art. 2. — *On Apparitions.* By John Alderson, M. D. Read to a Literary and Philosophical Society at Hull, January 1, 1805.

Dr. Alderson, while he allows the belief in ghosts, spectres, and apparitions to be well founded, has endeavoured to controvert the popular opinion of their existence depending upon supernatural agency, by bringing forward several instances where such appearances were to be accounted for by natural causes only, from some bodily disease in the persons by whom these spectres were seen. It is foreign to our purpose to enter into any metaphysical disquisitions; we may only observe, that in those cases he mentions, where a deceptio visus was produced by a temporary disorder of the animal functions, we will readily allow such appearances to have been fallacious, and the offspring of a distempered imagination. But this *negative evidence* is necessarily confined to those individuals mentioned. Dr. Alderson's explanation will go far to *diminish the actual number* of supernatural appearances; but the possibility of such an occurrence, or even the existence of it, cannot be completely overturned, unless he is prepared to disprove relations which have hitherto been considered as perfectly true and authentic. Have not some of these spectres, whether real or imaginary, occasionally uttered prophetic predictions, which have been afterwards realised? If we deny the reality of the spectre, we must allow the existence of a degree of prescience in the individual, which it is no less difficult to explain. Dr. A. admits it to be perfectly natural that Brutus should see a phantom; his imagination might be disturbed by "mental anxiety, inordinate ambition, or guilt;" but was it equally natural that, in this perturbed state, he should, at the same time, foresee the event of the battle of Philippi?

Art. 3. — *An Account of the Epidemic Dysentery which prevailed among the Dutch Troops at the Cape of Good Hope, in* 1804 *and* 1805*; with Remarks on the Use of Calomel in the Treatment of this Disease.* By Dr. Henry Lichtenstein, of Helmstadt.

This paper contains a good description of the epidemic dysentery, as it appeared at the Cape of Good Hope. The treatment at first was very unsuccessful, founded upon the stimulant plan of the Brunonian system. Calomel was at length given to the patients, at first sparingly, and afterwards more freely. "It was constantly observed, that the crises of the disease were accelerated by the calomel." The sequelæ of the disease were principally suppuration of the liver, induration of that viscus, and chronic diarrhœa. The Author mentions it as a remarkable fact, that the Hottentots accustomed,

customed to the climate, suffered much in the army, although better clothed, better fed, and more cleanly than in their agricultural occupations; while those who remained in the employment of the colonists, exposed to all the unwholesome weather, undergoing much fatigue, and being badly fed, were much less affected by the epidemic.

ART. 4.—*Case of Herpes Exedens Vermiculatus.* By A. B. GRENVILLE, M. D. Surgeon to his Majesty's Sloop Arachne.

THIS at first was a very troublesome and obstinate disease. Dr. Grenville produced a similar ulcer in a distant part by inoculation with the discharge. On examining a drop or two of the pus by an excellent compound microscope, he discovered in that fluid "a swarm of insects, of various length and size, and differently agitated."

"The cause of this scabies being now plausibly discovered, the therapeutic indication readily occurred to my mind. The sores were washed with diluted nitrous acid, for three consecutive mornings, and cerate applied, to defend them from external irritation. On the first of November they were anointed with ung. hydrarg. with which the cure of both the *original* and *artificial* eruption was finally obtained, within the space of another week. No allowance of spirits or wine was granted to the patient during his illness; and though entirely local, I frequently administered sweet acidulated beverages, and a grain of opium at night to faciliate rest."

ART. 5.—*Reply to Mr. Goodlad's Observations on Mr. Barlow's Theory "On the Origin of Urinary Calculi."* By JAMES BARLOW, Surgeon.

IN this Reply, Mr. Barlow endeavours to justify his former opinions, and to obviate some objections, and what he conceives to be misapprehensions on the part of Mr. Goodlad. The whole matter in dispute must be left to the parties themselves, who, most probably, as generally happens in controversies, will each retain his own opinion, unconvinced by the arguments of his opponent.

ART. 6.—*Convenient Method of constructing a Steam Bath, with an Account of its Effects in a Case of Gastritis.* By WILLIAM FORBES, Member of the Royal College of Surgeons in London.

IT is here proposed to convey steam by means of a tube communicating with a common tea kettle, and inserted into an aperture at the bottom of a slipper bath, in which the patient is to be placed; all the advantages of a warm bath may in this way be obtained, and more speedily than by heating sufficient water in which to immerse the body. Great relief was obtained by this bath in the case of Gastritis related, but the cure was effected by bleeding *ad deliquium* on the eighth day of the disease.

The Inquirer, No. 19.

After a fair and candid comparative view of the advantages and disadvantages, as to his means of improvement in the situation of

a Naval

a Naval Practitioner, the Inquirer concludes, that "if the list of naval surgeons be said to contain few illustrious names, the cause must be sought for somewhere else than in the nature of their situation." This is true so far as regards the opportunities they enjoy for increasing their medical knowledge, and correcting their theoretical opinions by accurate observations; but how few persons enter into the service, who have laid a sufficient foundation, whereon to erect a goodly superstructure, or who are endowed with that talent of observation so requisite for improving the science of medicine? A man of liberal education, and of real talent and abilities, finds nothing *inviting* in the situation of a sea surgeon; respectability of rank does not tempt him, and he is frequently subjected to treatment from persons of less cultivated minds, and less refined feelings, which at once mortifies and disgusts him. No prospect of future honours cheers his labours, and his sole motive of choice is generally a present maintenance. The service is also commonly entered into by youth, at an age when it requires no little degree of ardour and love of science to overcome the temptations to pleasure and indolence which exist around them. The following sentiments are so correct and judicious, that we cannot but repeat them, with a wish to impress them upon the mind of every one who is really solicitous for the improvement of medical science.

"In every large town there are professional men in extensive practice, who hurry from patient to patient, who write an old prescription because it is familiar to them, or a new one because it is fashionable, receive their fees, and think no more of their patient or his disease. That the public should suppose such men possessed of much experience is a natural mistake; but it is our duty to controvert the absurd idea, too prevalent among professional men themselves, that it is necessary to see very many patients to become an experienced practitioner, or that, in walking the wards of a crowded hospital, much practice is to be seen. On the contrary, a limited practice is most favourable for reflection and professional improvement. A multitude of patients only serves to distract the student, and to confound him; and the remarkable cases, which he may never again meet with, attract his whole attention, and lead him to neglect those common diseases, which, in future, it will be his chief employment to cure."

A Letter in Reply to the Report of the Surgeons of the Vaccine Institution, Edinburgh; with an Appendix, containing a Variety of interesting Letters on the Subject of Vaccination, and including a Correspondence with Dr. Duncan, Dr. Lee, and Mr. Bryce. By THOMAS BROWN, Surgeon, Musselburgh. Edin. 1809.

A Correspondence with the Board of the National Vaccine Establishment. By the same Author. London, 1810.

Mr. Brown's opposition to vaccination is sufficiently known by his former publication on the subject, and the present two indicate no abatement

abatement of zeal in the cause. Zeal, however, sometimes oversteps discretion, and that appears to have been Mr. Brown's case, if we are to judge by his correspondence with Mr. Bryce, which is here given; wherein Mr. Brown endeavours to evade the HOME questions put to him by Mr. Bryce, thereby admitting that some of his assertions in his former book cannot be maintained. As to the FACTS contained in that book, we cannot refrain from giving an extract of Mr. Welsh's Answer to Mr. Brown's Letter, soliciting information, and Mr. Brown's SINGULAR Reply thereto.

"I cannot conclude my letter, without expressing my regret that your Queries were not addressed to the medical practitioners here, prior to the publication of your book; the result of all the information I have been able to obtain in this town and neighbourhood being *completely at variance with that stated by you to have been received from this place.* I remain, &c.

"*Haddington.* "JOHN WELSH."

To this Mr. Brown replies,

"Far from regretting that I did not communicate with the medical gentlemen at Haddington, before I mentioned in my book that such cases had occurred there, I have daily reason to congratulate myself on the opinion I had formed of the extensive and alarming effects of system on the human mind; for had I done so, and afterwards *had been regulated by the information I would certainly have received, undoubtedly no such opinions would have been promulgated*, and which I now find many very respectable characters, both in and out of the profession, consider as entitled to attention and respect."

We think it unnecessary to enter minutely into a consideration of the main question; we would rather leave the decision of it to fair and candid discussion and future experience. We do not expect Mr. Brown's books to make many converts; defective arguments seldom derive much real strength from warmth of language, and personal abuse generally fails to operate conviction on the mind of an opponent. If Mr. Brown is *perfectly satisfied* of the justness of his own opinions, and is only solicitous for the public welfare, we do not see why he should be so very angry that the favourers of vaccination do not enter the lists against him, and defend what he thinks an untenable doctrine; or why he should *complain*, when speaking of the National Vaccine Board, that he "perceives in their conduct a design to allow the *facts themselves* silently and gradually to produce the extinction of the practice," seeing the "extinction of the practice" is the sole object he professes to have in view.

A Memoir on the Physiology of the Egg. Read before the Linnean Society of London, by J. A. PARIS, M. B. Physician to the Westminster Hospital. London, 1810.

THIS interesting and philosophical Essay commences with a few cursory observations on the opinion that all animal and vegetable productions proceed from eggs or seeds.

"The

"The extensive range which the *Ovipari* form in the scale of animated existence, renders the organization and developement of the Egg, a subject of great interest to the naturalist, whilst the hope of ascending to the source of vitality, by contemplating life at a period when the number and complication of its functions are the fewest, becomes a powerful inducement to the physiologist, to pursue the investigation; hence we find, that the philosophers of every age and nation have devoted much time and labour to this inquiry; but unfortunately for the earlier promotion of science, the influence of chemical powers in the scheme of animal life has but lately been duly appreciated; many beneficial results, however, have already attended this discovery, and the most exhausted topics of natural history have assumed new and unexpected aspects. The author therefore of this memoir, may reasonably hope to escape the censure which must otherwise have awaited the adventurer who could presume to beat the field, which has before been so ably explored by the indefatigable and united labours of Fabricius *ab aqua-pendente*, Harvey, Malpighi, Spallanzani, Hunter, and others equally illustrious.

"A powerful phalanx of philosophers maintain, with much plausibility, that the *Egg* * is the universal womb of nature, and that *oviparous* differ only from *viviparous* animals, by the latter breaking their bondage before they escape from the parent. Concerning the truth however of this opinion, which is comprehended in the popular aphorism, '*omnia ex ovo*;' or the success with which the eloquent Count de Buffon has levelled his shafts against the partisans of the ovular † system, I will leave to be decided by abler disputants. The observations which I beg to submit to your notice, do not involve either theory, but are connected only with those animals that are *oviparous*, in the common acceptation of the term—that is, *who deposit a germ to be developed by causes totally independent of parental influence* as such.

"Amongst the countless multitudes and varieties of animals, a very small proportion only produce living offspring; thus the immense tribes of birds, fishes, amphibious animals, and insects, with comparatively ‡ few exceptions, propagate their species by the intervention

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* *Egg*, the word *ovum* seems to be derived from the Greek word *oiov*, *solitarium*, because (unlike most *uteri*) it produces but one offspring.

† The system of the Ovarists has been preferred by Harvey, Steno, Malpighi, Valisnieri, Dulamel, Nuck, Littre, Swammerdam, Haller, Spallanzani, Bonnet, &c.

‡ Some fish are viviparous, E. G. *Muræna Anguilla*, or Eel, *Blennius Viviparous*, &c. Amongst the *Amphibia*, we may notice the *viper*, which brings forth its young alive, and hence probably derives its name, "*quod vivum pariat.*" Spallanzani considers also the production of frogs as rather of a viviparous than oviparous nature; this rudiment however of the future animal, certainly partakes as much of the nature of an egg as of

tervention of the egg; nor is this mode of generation either accidental or unimportant. Had the winged inhabitants of the air been *viviparous*, the burthen of gestation would have impeded the action of their wings, and so far increased their gravity, as to have rendered them incapable of the exertion of flight. The rigid and unpliant coverings of *crustaceous* animals, would oppose the expansion necessary for the growth of a fœtus; and it is evident, from the structure and habits of the tribe of serpents, that if they had been *viviparous*, their offspring must, from their tortuous flexions, and the friction arising from their progressive motion, have been constantly exposed to injury and destruction; and lastly, the *multiparous** nature of insects and fishes, at once convinces us how impossible it would be to engender them by any other mode than that which nature employs."

As the author confines himself in this memoir to the physiology of a particular description of eggs, he proceeds to lay down his distinctions and definitions of the several component parts of the eggs of birds, and the progress of their formation.

" The eggs of oviparous animals admit of an evident division into two classes, which I shall name. I. The PERFECT, and II. The IMPERFECT. The former, which are deposited by the *Aves*, and some genera of *Amphibia*, are completely covered by a hard shell, or membrane, and receive no additions after their exclusion; whilst the latter are deposited by most *Pisces*, and in general constitute a soft mass, not being protected by any external involucre. The observations contained in this memoir, relate more particularly to the egg of birds, their history however comprises whatever is interesting or important in the germs of inferior animals.

" In order that I may be better able to form a systematic relation of those new facts I wish to introduce to your notice, I shall briefly relate the successive operations by which the egg is formed in the body of the animal; the necessity of a more diffuse description,

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of a fœtus, and may probably be considered as a connecting link between the two great classes. Insects likewise present us with exceptions, and several whimsical varieties; the *Aphides* for instance, lay eggs at the end of Autumn, when the young produced from them in the Spring, are viviparous during the whole Summer! the *Cocci* hatch their eggs before exclusion, and the young force their way through the abdomen of their mother; the *Onisci* carry their eggs in a particular receptacle, from which, in process of time, the young escape.

* Reaumur ascertained, that a single queen bee had laid, in the months of March and April, 12,000 eggs; and Lewenhoeck found that the *Musca carnaria* deposited 144 eggs, from which, in one month, were produced as many flies; so that supposing one half of these to be females, there would be, in the third month, 746,496 flies. The amazing fertility of fish may be illustrated by the *Gadus morhua*, or cod-fish, which will deposit among the rocks 9 or 10 millions of eggs; and again the perch, *Perca fluviatilis*, produces in April and May, 281,000 ova!

tion, is superseded by the minute and valuable details which may be found in the writings of Harvey and Malpighi.

"The rudiments of the *ovum* are first visible in the *ovarium*, which, in fact, is nothing but a congeries of *vitelli*,* or yolks, attached to the spine by a proper membrane; this repository is denominated, by Fabricius, the *vitellarium*, or *vitellorum racemus*, and may be considered as analogous to the *ovarium* of the *mammalia*, or to the *roe* of fishes. These *vitelli*, or *yolks*, generally vary in progression, from the size of a millet seed to that of an acorn; each according to its maturity, is successively detached from the rest, whence it descends a tube, called (from its resemblance to a funnel) "*infundibilum*," and arrives at the *uterus*, the internal surface of which is extended by spiral convolutions; here the albuminous fluids are secreted and transmitted to the egg, during its passage to the *fundus uteri* and *cloaca*, where it receives its external crust from the calcareous matter which is there deposited on it.

"The egg thus formed and completed, possesses every essential for its subsequent maturation, and requires only the emphatical agency of heat for its developement; this, in the different genera of animals, is conveyed by different media; in birds it is applied through incubation,† but in the *amphibia*, and other animals, the heat of whose bodies is inconsiderable and inefficient, the eggs are deposited in mud or sand, or exposed to the rays of the sun, by whose prolific influence myriads of beings are daily called into life and activity; or they are placed in other favourable situations, all of which are well known to the disciple of Linneus. It is however worthy of remark, that the medium through which heat is applied, is suitably varied in the same genus in different climates; thus, in Senegal, the ostrich abandons her eggs to be hatched by the burning sands, whilst, in the more temperate and congenial regions of the Cape of Good Hope, like other birds, she is inclined to incubation.

"The different species of *Æstrus* will afford us an illustration of the variety of situations in which the *ova* of animals are deposited; the *œstrus bovis*, or gad-fly, deposits them under the skin on the backs of oxen; the *œstrus hæmorhoidalis* in the rectum of horses; and the *œstrus ovis* in the frontal sinus of sheep, &c.; the excrescences also which are to be seen on cabbages, turnips, and other plants, always contain the egg of some insect. This faculty which animals possess of selecting an appropriate nidus for the reception of their ova, is truly astonishing, and affords a most beautiful illustration of the wisdom and intelligence which nature displays in the adaptation

* *Vitellus*, deriv. a *vita*, because it contains the embryon.

† There are also other animals which accelerate the volution of their ova, by incubation; thus Bees in a hive, generate a considerable quantity of heat, without which their eggs would perish; and the *Testudo Mydas*, or common Turtle, deposits her eggs in the sand, and incubates during the night.

adaptation of means for the accomplishment of her ends: whether however the animal is directed in its choice by a moral or physical feeling, is not so evident; that is to say, whether it results from foresight, or from the blind obedience of a law of nature, which wisely connects the wants of the parent with the interests and welfare of the offspring; thus, for example, many animals, altho' termed *amphibious*, never resort to the water but to procreate their species; now are we to conclude that they repair thither because they know that this element alone can unfold their ova, or because, by so doing, they gratify some internal sensation, which renders their abode on dry ground unpleasant?"

A general account of the constituent parts of a *perfect* egg having been already given, this ingenious and diligent naturalist enters on the detail of its anatomy, if we may be allowed the expression.

"The parts of which the *egg* consists are, 1. *Vitellus*, or yolk, with its *capsule* and *cicatricula*; 2. *Albumina* with their proper membranes; 3. *Chalazæ*; 4. *Folliculus aeris*; 5. *Common membranes*; 6. *Exterior involucrum*, or shell; to each of which we will successively direct our attention.

"The *vitellus*, or yolk, is the part formed *in vitellario*, and is a yellow fluid contained in a membranous capsule, in which a white circular disk * is discernible; this is named *cicatricula*, and is the speck of entity, the germ that is to be developed into the animal. "In hujus gratiam," says Malpighi, "reliqua comproducta videntur." We here then have arrived to the earliest stage in which we can detect the animal's existence, our imperfect faculties will not allow us to ascend farther, and yet, even now, the body is formed, as the experiments and observations of Malpighi† most clearly demonstrate. The yolk is surrounded by a more tenacious fluid, to which the name of *albumen*, or commonly the white, has been assigned; this may easily be divided into two separate and distinct portions, each of which is contained in a concentric membrane; they differ from each other considerably in specific gravity, and seem to answer different purposes in the economy of the egg; the consistence of that which is exterior, is far less than the one which immediately envelops the yolk, and is consumed in the earlier periods of incubation,‡ whilst the internal and more viscid *albumen* seems reserved for the latter stages, when the chick must require a greater proportion of *generative matter* than at any other period of its development. Many of the ancient philosophers imagined that the chick was formed out of the yolk, and that the white afforded nutriment; such a theory must however be at once abandoned, when it is known that the *vitellus* suffers no change by incubation, except

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* Fabricius supposes it to be a vestige of the ruptured *peduncle*, or that portion of membrane by which each yolk is connected to the vitellarium, and Æmilius Parisanus contends that it is the semen of the male.

† Vid. Marcell: Malphighi "de form: pull; in ovo."

designed to administer support, until its digestive organs can gain sufficient powers to perform their functions, and the beak a degree of firmness adequate to withstand the hardness of its natural food, "Ipsum animal," says Pliny, "ex albo liquore ovi corporatur, cibus ejus in luteo est;" doubtless however the *vitellus* answers also other purposes, or why should it be necessary to those birds * whose parents so sedulously supply them with nourishment?

"At each end of the egg, a white shining body is inserted into the capsule of the yolk, which extends into the *albumen*, in which it floats; these, from their resemblance in colour and transparency to *hail*, have gained the name of *chalazæ* or *grandines*, and, from having been formerly regarded as the sperm of the male bird, or *treddles*. Bellini supposes that they are composed of numerous canals, which open into the *amnios* or *cicatricula*, and send out their roots into the white, for the purpose of forming a communication between them; Munro however observes, that, "if they be canals, they cannot have the least communication with the cavity in which the chick resides, at any time, or in any state of the egg, otherwise than as they are both adhering to the membrane of the *vitellus*, upon which, or within which, no particular fibres, no canals, are stretched to the *cicatricula*." The *chalazæ*, says Harvey, appear to be the poles of the microcosm, and serve to connect the different parts of the egg, and to retain them in their due position; in addition to such an office, Denham ingeniously conjectures that as they divide the yolk into two distinct and unequal hemispheres, they will preserve the *cicatricula* (let the position of the egg be what it may), in the same situation; for since the *chalazæ* are specifically lighter than the white, the yolk is kept buoyant, and the *cicatricula*, as it resides in the smaller hemisphere, will be always uppermost; this, in my opinion, is the true theory of their use, for such a structure will not only preserve the *cicatricula* from the dangers of concussion, but by regulating its distance from the source of heat, it will ensure to it a more completely uniform temperature than could otherwise happen, and which is so essential to the evolution of the animal, that the smallest irregularity overthrows the nice balance of the different actions that are to mature it, and produces fatal effects. So solicitous therefore was nature to rescue the germ from the consequences of cold, that she has ordained other provisions, which seem equally as effective as the *chalazæ* for the preservation of its proper temperature; thus the *cicatricula* is on all sides surrounded by fluids, which are feeble conductors of caloric, these must therefore powerfully retard the escape of heat, and prevent the destructive chills which the occasional absence of the parent might otherwise induce. The eggs of

* Pigeons, for example, whose crops John Hunter ascertained to secrete a peculiar fluid during the breeding season, for the sustenance of their young.

many animals appear protected by an analogous apparatus; thus the ova of frogs, and other amphibia, are enveloped in spheres of mucilage, which the experiments of Spallanzani shew to be essential, as he found when this gluten was removed, the egg immediately perished. It is an evident fact that those fishes who retain their vitality long after their removal from the water, as eel and tench, have the power of secreting a slimy fluid, with which they envelop their bodies; whilst, on the contrary, those which, when drawn on shore, quickly die, as for instance, mackrel, possess no such faculty, or at least only in a small degree; is it not therefore extremely probable that this albuminous matter, by repressing evaporation, and preventing (like the fluids of the egg) the escape of heat by its non-conducting nature, is the principal cause of this peculiar tenacity of life?*

"The hen bird seems herself conscious of the mischief that would accrue from an irregular or decreased temperature; she is often seen to make use of her bill to push to the outer part of the nest, those eggs that were nearest the middle, and to bring into the middle, such as lay nearest the sides. The Egyptians † however, by a nice adjustment of the temperature of their ovens, succeed in hatching a great proportion of the eggs subjected to artificial heat. The celebrated Reaumur introduced this method into France, and Sir James Hall has lately invented a regulating stove, by which an equable temperature for this purpose may be easily and advantageously procured. Sir Busick Harwood, the ingenious Professor of Anatomy at Cambridge, has frequently attempted to develop the egg, by the heat of his hot bed, but has succeeded only in producing monsters; the evident result of an unsteady application of heat. It must however be remarked, that deviations from the correct temperature, are injurious and fatal only in proportion to the degree of vital energy which the ovular embryon possesses; thus we learn, from the experiments of Spallanzani,‡ that the eggs of insects are better able to sustain the vicissitudes of temperature than those endued with more exalted vitality; thus it is, that the eggs of cold blooded animals bear with impunity such an increase or decrease of temperature, as is sufficient to destroy the animals themselves; for Spallanzani found tadpoles

* Perhaps a prodigious accumulation of fat may also, sometimes, have a share in producing this effect; thus the *Silurus Glanis*, which is the fattest of all fresh water fishes, as it grows to the weight of 300 lb. lives very long after being taken out of the water.

† The inhabitants of the single village Berme, amongst whom only this art is practised, give life, by means of their ovens, to two-thirds of the eggs intrusted to their care, amounting in one season, which continues but for six months, to the astonishing sum total of 92,600,000. Corneille le Bruyn, tom. 2, has collected the observations of many travellers on this subject.

‡ Spallanzani. Tracts on the nature of animals and vegetables.

tadpoles and *frogs* to perish at 110°, bnt their eggs only at 133°. If we pursue this inquiry, and quitting the animal kingdom, descend into the scale of vegetable existence, where the energies of vitality are still more feeble and obscure, we shall find the same relative power of sustaining heat or cold between the plant and the seed, as we recognized between the animal and its egg.

(To be continued.)

An Account of Diseases in an Eastern District of London, from June 20 to July 20, 1810.

ACUTE DISEASES.	
Peripneumonia Notha	3
Cynanche Tonsillaris	5
Dysenteria	2
Rheumatismus Acutus	3
CHRONIC DISEASES.	
Tussis	10
Dyspnœa	5
Tussis cum Dyspnœa	4
Hæmoptysis	5
Phthisis Pulmonalis	3
Catarrhus	4
Pleurodyne	3
Anasarca	2
Ascites	1
Enterodynia	4
Menorrhagia	5
Fluor Albus	7
Amenorrhœa	3
Procidentia Ani	1
Epilepsia	1
Urticaria	2
Rheumatismus Chronicus	12
PUERPERAL DISEASES.	
Ephemera	3
Menorrhagia Lochialis	5
Mastodynia	6
Rhagas Papillæ	3
INFANTILE DISEASES.	
Febris Mesenterica	2
Erysipelas Infantile	1
Vermes	4
Convulsio	3
Pertussis	6

During the last few weeks a considerable change has taken place in the state of the weather. After a very long continuance of Easterly and North Easterly winds, and an almost total absence of rain, in consequence of which, the surface of the earth assumed a most dry and sterile appearance, the occasional changes of the wind to the West and South West, together with some very copious showers of rain, have produced a favourable appearance, and given the hope of a more plentiful supply of the necessary articles of food both for man and beast. This alteration has also proved favourable to the human constitution, and some diseases which were aggravated by the long continuance of the cold winds have now assumed a milder form. Still, however, the very quick alternation of heat and cold has exposed many persons, especially those of a delicate habit, to considerable inconvenience. The difference of temperature,

rature, even at the same hour, under the influence of the sun's ray, and in a situation exposed to the Eastern winds, which have still in some degree prevailed, has produced unpleasant effects. Pains of the face and teeth have been very common. These might be in some instances traced to a carious tooth, the removal of which has proved a radical cure; but in other cases they have appeared to be a symptom of rheumatism. This disease, as it is well known, appears in such a variety of forms, and is so connected with some peculiarities of constitution, that it is sometimes not easy to detect it, and the cure is at all times tedious and difficult. Affections of the throat have been very frequent during the period referred to. Cynanche tonsillaris has appeared in several instances, but in a milder form than it sometimes assumes. It has been rather a local than general disease; even where the tonsils have been considerably enlarged, and where difficulty and pain of deglutition have been felt, the degree of fever has been inconsiderable. The use of a domestic gargle, and the opening of the bowels with small doses of cathartic salts, have been the only means that were found necessary to the cure.

Report of the Diseases of Edinburgh for May, 1810.

By John Roberton, M. D.

Before the commencement of the month, the weather had become dry, and it continued so during about one-third of it. Till this time, when we had some gentle falls of rain, vegetation was so parched as to be almost completely arrested in its progress. From this period till the termination of the month, we had frequent falls of rain, and some thunder storms.

The barometer, during the early part of the month, stood high, and even during the succeeding rains, it did not fall considerably.

The thermometer stood in general from 55 to 70, and, once or twice, it rose even several degrees higher.

During this period, there scarcely seems to have been any one complaint of such a prevalent or troublesome nature as to require any very particular description. There were various slight affections, such as catarrh, cynanche tonsillaris, &c.; also some cases of fever, which are indeed, never absent from this place; some few remaining cases of chin-cough, and a few of hydrocephalus internus.

This scarcity of disease among us seems to be owing partly

partly to the nature of the season, and partly to the great number of families which have retired into the country; at which times they carry with them not only the healthy but also the sick.

INTELLIGENCE.

The Editors have the pleasure to insert the further proceedings of the Lincolnshire Benevolent Medical Society, on reformation in medical education and practice. As the Bill, it is understood, will certainly be introduced into Parliament early next winter, they are very desirous to recommend the subject to the attentive consideration of their readers, and for that purpose the Medical and Physical Journal will be open to the temperate observations of the Faculty, whether they be favourable or adverse to the plan.

"*Bull Inn, Horncastle, July* 19, 1810.

"At the Anniversary of the Lincolnshire Benevolent Medical Society, held this day, SAMUEL PITTINER, Esq. President, in the Chair: Dr. HARRISON having laid before the Meeting a printed 'Sketch of the proposed Bill for the Improvement of the Medical and Surgical, and Veterinary Sciences, and for regulating the Practice thereof,' and having given an account of the powerful encouragement and support which the Plan received during his two last visits to London, from many persons of great professional and political consideration:

"*Resolved, unanimously*,

"1st. That this meeting is much pleased to find, that in consequence of inquiries first instituted in Lincolnshire, and subsequently extended throughout the United Kingdom, it is now generally admitted, that legislative interference is become indispensible to establish the proper distinction between well educated practitioners and others, who have assumed their titles, and filled their stations, to the great injury of society and the regular faculty.

2dly. This meeting is of opinion, that it is desirable to procure better information of the present state of medical practice, being convinced that such information, if complete, would establish beyond all controversy, the great and increasing necessity for some proof of professional competence being required from all future candidates for medical employment.

3dly. This Meeting having carefully examined the printed sketch of the Bill,* and compared it with the manuscript copy submitted to them last year, are confirmed in the unanimous sentiment then expressed—that the Bill by establishing a National Register of accredited Practitioners, by providing for the careful admission of future members of the profession; and by increasing the opportu-

* The Bill, divested of legal form, to bring it within the rules of Parliament, has been printed, and is published, together with Dr. Harrison's "Address to the Lincolnshire Benevolent Medical Society, &c."

nities for medical instruction, would, if carried into a law, become the foundation of a gradual and effectual improvement in medical science and practice.

4thly. "That the Thanks of this Meeting be given to Dr. Harrison for his unwearied exertions in the business, and that he be requested to take such further steps as appear best adapted to carry into effect this desirable object."

The following report was read at the last annual meeting of the Governors of the London VACCINE INSTITUTION.

"The Board of Managers have the happiness to inform the Annual Meeting, that the cause of Vaccination still continues to flourish under their auspices. This is evinced by the numerous applications for matter, not only in the Metropolis, but from all parts of the United Kingdom, the colonies, and countries abroad.

"They have great pleasure in observing the increasing progress of vaccination; their returns being greater than on any former year. It appears that since the last annual report, there have been inoculated by Dr. Walker 2,087; from the beginning 6,105. By the appointed inoculators in the metropolis, last year 1,105; from the beginning 2,163. By the appointed inoculators in the country, last year, 54,665; from the beginning 156,573.

"Dr. Walker, since the last report, has supplied to 3,973 applicants, 19,865 charges of matter. From the beginning, to 12,361 applicants, 61,088 charges.

"In having been enabled to effect the foregoing incalculable services to society at large, the board of managers have to acknowledge a liberal support from a generous public; yet they owe to that public, and to the cause of vaccination, the statement of the fact, that the support of this most extensively useful establishment requires still further contributions, to enable the managers to completely effect the vast patriotic and philanthropic plan.

"It is to be lamented, that the fatal malady which has, during the last thousand years, committed ravages the most dreadful in every quarter of the world, is not yet extinct in the metropolis. The board of managers, however, request the attention of every member of the institution to this most important and gratifying circumstance; namely, that while 1163 have died of the small-pox, during the last year, as appears from the bills of mortality, not a single instance of the kind is represented as having occured from vaccination."

At a late meeting of the Royal Society, the translation of a paper by M. DELILLE was read, describing the real nature and properties of the celebrated Bohan Upas, or poison tree of Java. The author, a French physician, and a member of the National Institute of Egypt, transmitted this paper from the East Indies to the Royal Society, by an English lady. The botanical account of the plant in question he received from one of the French naturalists who accompanied Captain Baudin, and who resided some time in Java, where he visited the interior of the country, and with much difficulty

difficulty prevailed on the natives to show him the different poison plants, which they carefully conceal, for the purpose of using them in war. Hence the many fabulous accounts that have been circulated respecting the fatal influence of the *Upas*; which in the language of the Javanese, signifies vegetable poison, and is applied only to the juice of the *Bohan* tree, and another plant with a twisted stem. The former is a large tree, which the writer considers as a new genus; the latter, yielding an equally powerful poison, is of the woodbine family. The *Upas*, or juice, is extracted by an incision made in the bark with a knife, and being carefully collected, is preserved by the natives to be employed in their wars. As to its diffusing noxious effluvia in the atmosphere, and destroying vegetation to a considerable distance around it, the absurdity of these stories is sufficiently exposed by the fact that the climbing species requires the support of other plants to attain its usual growth. Dr. Delille made several experiments with the upas on dogs and cats. An incision was made in the thigh of a dog, into which were dropped eight grains of the juice. The dog soon began to vomit, and continued vomiting at intervals till he became convulsed, and died in twenty minutes. Six grains were put into the thigh of another, which was seized with the same symptoms, and died in fifteen minutes. A cat was treated in like manner, but the effects were more speedy and powerful; she expired in a few minutes. All these animals died howling and in great agony. The author also made several experiments on the effects of this poison when applied internally. A grain and a half being introduced into the stomach of a dog, produced only a slight purging. To another were given four grains, which in about four hours produced the same effect, together with vomiting, and the dog died in the course of half a day. On examining the bodies of these animals after death, no very extraordinary appearances were discovered; the ventricles of the heart were full of blood, and some slight traces of inflammation appeared in the stomach; but the derangement was not so great as might have been expected from such a violent and sudden death. From this circumstance the author concluded that the absorbents had transmitted the poison to the nerves of the stomach, and that this peculiar species of vegetable poison acts exclusively on the nerves.

The MEDICAL SOCIETY of LONDON have in the press a volume of Memoirs, containing several valuable Communications in Medical and Surgical Science from eminent resident and corresponding members of the Society. The title of the volume will be, "Transactions of the Medical Society of London, Vol. I. Part I." and it will be accompanied by engravings. Part II. will appear in a few months afterwards, the Society having come to the determination of giving publicity to their Transactions more frequently than heretofore.

A new edition, being the fourth, of DR. TROTTER'S Essay, Medical, Philosophical and Chemical, on Drunkenness, and its Effects on the Human Body, with many additions, is just published.

Meteorological Diary, kept at Clapton, Hackney, from March 27 to April 24, 1810, by THOMAS FOSTER, Esq.

Day		Therm. Max.	Therm. Min.	Barometer Max.	Barometer Min.	Wind.	Weather.
March	27	51°	41°	29 80	29 65	S. S. E.	Clouds & rain
☾	28	52	35	29 94	29 89	W.	Sun & showers
	29	55	34	29 99	29 95	W.	Clear & clouds
	30	54	41	29 96	29 80	S. W.	Fair day
	31	51	39	29 80	29 49	S.	Fair—rain
April	1	51	41	29 72	29 55	S. E.	Fair
	2	48	42	29 80	29 67	E.	Small rain—fair
	3	57	40	29 67	29 41	S. W.	Small rain
●	4	49	33	29 77	29 45	N.—S.	Showers
	5	51	43	29 67	29 49	S. W.	Clear—cloudy
	6	42	41	29 34	29 33	S.	Sun & showers
	7	52	38	29 51	29 40	S.	Sun and showers
	8	54	42	29 52	29 50	S. W.	Fair, *a. m.*—rain, *p. m.*
	9	51	42	29 56	29 48	S. E.	Rainy
	10	45	33	29 70	29 66	N.	Rainy
◑	11	43	31	29 92	29 72	N. E.	Showers
	12	40	29	29 95	29 95	N.	Fair
	13	43	32	29 95	29 92	N.	Cloudy—some snow
	14	49	37	29 86	29 84	N. W.	Fair
	15	52	37	29 77	29 43	S. W.	Sun and clouds
	16	51	37	29 69	29 50	S. E.	Clear, *a. m.*—small rain
	17	54	41	29 69	29 67	S. W.	Sun and showers
	18	59	42	29 80	29 65	S.	Fair
	19	60	37	29 98	29 80	S.	Fair
	20	61	43	30 20	30 15	S. E.	Fair
	21	63	42	30 20	30 20	W.	Fair
	22	63	42	30 20	30 20	W.	Fair
	23	66	41	30 26	30 20	W.	Fair
	24	64	38	30 26	30 21	S. E.	Fair

BAROMETER.			THERMOMETER.		
Highest, April	24	30.26	Highest, April	23	66
Lowest, ditto	6	29.33	Lowest, ditto	12	29
Mean - -	29	29.79½	Mean - - - - -		47½

OBSERVATIONS.

March 30. *Cirro-Cumuli* observed in the sky in the morning.

April 6. The maximum of the Thermometer happened at 11 o'clock, *p. m.*

8. *Salices*, Vitellina, Fragilis, Rubra, Viminalis, Velutina, Russeliana, and others in flower

14. The modification of cloud, called *Cirro-Stratus*, observable in sky.

15. *Cirro-Cumuli* observed

16. *Cirri* in short tufts pointing to the South East, succeeded by rain.

17. *Cirro-Strati* in the sky.

18. *Cirro-Cumulus* prevails.

19. Very hazy moon.

20. The wryneck *Jynx Torquilla* first heard. I observed the swallow, *Hirundo Rustica*, as well as the martin, *Hirundo Urbica* flying about in the neighbourhood of West Ham.

21. Swallows first seen at Clapton.

22. *Cuckoo* first heard.

23 *Cirro-Strati* prevail.

24. *Cirro-Cumuli* observable.

By a Hygrometer of a peculiar construction which I make use of (see an account of it in Phil. Mag. for November 1801,) it appears that the air has been extremely dry ever since March 30th.

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ADVERTISEMENT.

IN undertaking to conduct the Medical and Physical Journal, the present Editors think it incumbent on them to express their sincere desire to promote the objects of Medical Science. This Journal, from its commencement, has experienced a degree of patronage which assures them, that a publication which conveys the earliest information upon medical subjects, is well received, if it be ably and faithfully executed.—The advantage of obtaining monthly, in the remotest provinces, intelligence of every thing that is interesting to the profession, almost as soon as it transpires in the Capital, and of receiving accounts of medical affairs from every part of the civilized world, is too obvious to require demonstration: but on this account, allowance must be made, if the communications are not always of equal importance. The nature of the work scarcely admits of elaborate essays, or of profound and extended critiques. These must be sought for in the transactions of societies, in the annual, and in the quarterly medical publications, which are conducted in a manner, and with a spirit, that evince a decided advance in the art of late years in this country, where improvement in practice, and discoveries in science, are continually rewarding the exertions of the experimenter.

The views of the Editors of the Medical and Physical Journal will, therefore, be steadily directed to give publicity to such cases and facts as must interest every man who is desirous of being acquainted with what is passing in the medical

medical world, and they trust that the value of these will augment, by the number of their correspondents becoming more respectable.

In the critical department also, they hope in future to give a satisfactory account of such new medical publications as may be interesting; and in executing this part of their labour, they feel anxious to merit the character of being correct and impartial.

As discussion is calculated to improve knowledge and establish principles, the pages of the Journal will be open to the examination of opinions; but the Editors wish it to be understood, that they are decidedly hostile to personal allusion and illiberal disputation.

In the present state of continental affairs, they cannot hope to obtain regular supplies of foreign journals; but they will neglect no opportunity of making their readers acquainted with the medical literature of other nations, and in this month's Journal they have the satisfaction to insert some interesting articles from the New York Medical Repository, a work which enjoys high and well-merited reputation in America. Finally, they have to acknowledge the politeness of numerous professional friends who have offered to assist them in the prosecution of a work, of which the success materially depends on the character of the communications it contains.